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THE CORRESPONDENCE OF CADWALLADER COLDEN AND HUGH GRAHAM ON INFECTIOUS FEVERS (1716–1719)

SAUL JARCHO

Between 1868 and 1918 the New-York Historical Society published twelve volumes (1, 2, 3) containing letters and other writings by Dr. Cadwallader Colden (1688-1776), the eminent colonial philosopher, mathematician, botanist, historian, physician and government official. This handsome series of publications does not include the full diapason of that productive and capable man. There remains a considerable quantity of material, which is listed by title only in the seventh volume of the *Colden Letters and Papers* (4). This unpublished part of the collection contains the correspondence between Cadwallader Colden and Hugh Graham which forms the basis of the present essay. The letters afford valuable insight into the condition of medical thought and medical practice in colonial Philadelphia and throw incidental light on the early development of one of America's first important scientists.

Cadwallader Colden was born in 1688 in Ireland, probably at Enniscorthy (5), County Wexford, where his father was a Presbyterian minister. He received the degree of Master of Arts from the University of Edinburgh in 1705, then studied anatomy and chemistry in London. In 1710 he migrated to Philadelphia, since he did not have enough money to start

practice in London. In Philadelphia he divided his time between medicine and the importation of commodities from the West Indies. He went back to England and Scotland in 1715 and returned to America in April, 1716, as the correspondence with Graham indicates. In 1718 he paid a visit to Governor Robert Hunter of New York, who invited him to establish himself permanently in that province and to embark on a career in government. He accepted the offer. His subsequent work as surveyor general, member of the council, and lieutenant governor, as well as his work in history, philosophy, medicine, and natural science is discussed in detail elsewhere (5, 6). He died in 1776.

Concerning Hugh Graham, Colden's friend and correspondent, very little information has come to light. The letters published in the present study, with the addition of one letter from Colden to Graham (7) which has been omitted and one letter (8) published in the *Colden Letters and Papers*, constitute almost everything that has come to light thus far, except for a few minor mentions (9).

One of the published letters (8) contains two hints about Graham's life story. In January 1719, Graham wrote to Colden: " You are to know that in Octob: last I thought fit to take unto me a wife, after I was a moneth married I fell ill of a feavor . . . this is not all, I have yet a verry bad cough, and frequently spit up yellow and greenish stuff from my lungs, and now and then a chalky or tartarous substance. . . . The Colledge of Physicians here have determin'd to put the laws in Execution against all Physicians that will not come in to their Colledge, and all the forreign Graduates has been summon'd to come in, many of us resolve to come in . . ."

This quotation suggests (a) that Graham may have had empyema or tuberculosis (b) that if he had tuberculosis he may have caught the disease from his wife (c) that if he had tuberculosis he must have died soon afterward, since the symptoms were severe (d) that he may have been a graduate of a foreign medical school, presumably the University of Leyden.

A search of the roster of graduates of Leyden (10) reveals a record of the enrollment of " Hugo Graham, Scoto-Brittanus " under date of February 20, 1711. The Roll of the Royal College of Physicians of London (11) contains no record of anyone named Graham. Perhaps Hugh Graham had died of tuberculosis before he could carry out his plan of joining the College.

Scope and Content of the Correspondence

The entire extant Colden-Graham correspondence consists of seven letters:

1. Colden to Graham, May 25, 1716, transcribed, *vide infra.*
2. Colden to Graham, October 1716, transcribed, *vide infra.*
3. Graham to Colden, February 1716/7, transcribed, *vide infra.*
4. Colden to Graham, June 1717, transcribed, *vide infra.*
5. Colden to Graham, December 1718, a letter dealing with Indians, not transcribed in the present essay.
6. Graham to Colden, June 15, 1719, not transcribed in the present essay; printed in *Colden Letters and Papers,* vol. 1, pp. 97-98.
7. Colden to Graham, undated letter [1719?] in reply to preceding; transcribed, *vide infra.*

Content of the Individual Letters

1. Colden to Graham, May 25, 1716. This is clearly the initial letter of the series and was designed to continue the friendship interrupted by Colden's departure to America. The personal section of the letter alludes to business transactions between Colden and Graham and also describes Colden's effort to obtain the latest books on credit from a London dealer.

These discussions are followed by an essay of about 800 words entitled "An Account of the Fever . . . on Board the Gloucester Galley of London . . . 1716." This is in all respects a typical eighteenth century performance. The concise clinical description occupies only about one-eighth of the text. It deals chiefly with the time of onset, the duration of the fever, the condition of the pulse, and the appearance of the urine.

The remaining seven-eighths of the little essay is an attempt to show how far Colden's theorems, based on an apparently original doctrine of secretion, were applicable to the outbreak of fever (influenza?) aboard the *Gloucester.* According to these theorems, the blood in cases of fever is thick and excessively abundant. The purpose of treatment should therefore be to lessen the quantity by evacuating the gross parts and to dilute the remainder.

The fever patient should be kept cool in order to hinder excessive evaporation caused by the febrile violence of the circulation. We have here an early statement of the "cooling regimen" which was later to become associated with Colden's name (12). We are also given to understand

that the idea of such a regimen was a corollary to the aforementioned doctrine of secretion.

Another of Colden's rules was never to produce evacuation by purges or sweats as long as the urine was free from sediment. Instead, the patient was bled freely and given fluids. Medicines were used sparsely. Colden felt that the application of his principles in the *Gloucester* outbreak had given satisfactory results. It is regrettable that his letter does not contain a full description of his theorems and that the earlier manuscript on secretion, which apparently was entrusted to Halley, has been lost. The influence of Bellini and Santorio is clearly marked, and the arrangement of the doctrines in the form of theorems suggests the principles of Newton. There is much evidence in Colden's later writings of the influence of these three men.

2. Colden to Graham, October 1716. This letter starts with a brief vignette of the famous William Douglass (1691-1732), a sensitive truculent Scot, who was later to gain renown for his essay on *Epidemical Eruptive Miliary Fever* (13).

The important section of the letter consists of a few comments on an outbreak of smallpox which had occurred in Philadelphia during the summer of 1716. According to Colden the disease had been absent for 14 years and was now almost universal. Many of the patients had diarrhoea. This complication was welcomed, since it was believed to lessen the peccant matter and to prevent the development of putrid fever. Colden also felt that the skin lesions would never fill as long as the fever continued, because the filling was caused " by a separation of ye gross parts " of the blood and this process was prevented by the high speed of the blood current allegedly caused by fever.

Colden makes the interesting incidental observation that the domestic fowl were suffering from pox (14) and that many had died.

3. Graham to Colden, February 1716/7. This letter describes a case of smallpox complicated by mania and a case of Laennec's cirrhosis(?) with autopsy.

4. Colden to Graham, June 1717. The opening paragraph of this fragment adds to our knowledge of Colden's ideas about body fluids. In smallpox, he says, if the subsidence of facial swelling is not followed by swelling elsewhere, the gross components which have been stagnating in the face will re-enter the circulation, thicken the blood, and cause fever. These components should therefore be evacuated by purgation.

The remainder of the letter is devoted to speculations concerning

diseases of the head, which had been exceedingly common during the recent rainy season. Colden's thoughts on the subject have a Galilean or Torricellian appearance. He states that the parts of the blood which are carried to the head are lighter than the rest. In a light air—by which he perhaps means during periods of low barometric pressure—the blood is especially light, hence the head is especially likely to be affected. Since the light parts of the blood are best evacuated by stool, evacuation is the proper treatment for cerebral disorders.

This line of reasoning is obviously an attempt to explain an observed peculiarity in the seasonal incidence of disease. The basic fact had not been checked with care and amounts to no more than a casual clinical impression. The explanatory hypotheses likewise were submitted to no searching critique.

It is not alone the crudity of Colden's reasoning which makes his discussion seem remote and fantastic. We are living in an era in which relatively little attention is lavished on the *explanation* of climatic and seasonal vagaries of disease. Textbooks of medicine routinely mention the gross seasonal incidence of individual diseases but rarely—except for diseases involving arthropod vectors—suggest that such peculiarities of incidence merit investigation. The contemporary physician has discarded meteorology along with botany and pharmacognosy. But Time's wheel will turn again.

5. Colden to Graham, June 1717. This letter is not transcribed in the present study. It deals with the religious beliefs of the Indians.

6. Graham to Colden, June 15, 1719. This letter, not transcribed in the present study, has been printed in the *Colden Letters and Papers*, vol. 1, pp. 97-98. It mentions Graham's marriage, his alarming pulmonary symptoms and the turbulent medical affairs of contemporary London, including a scurrilous controversy between John Woodward and Richard Mead. The letter also contains an indistinctly worded indication that Graham had studied medicine abroad.

7. Colden to Graham, undated letter [1719?] in reply to preceding. This letter acknowledges the receipt of a new book by Woodward, evidently *The State of Physick, and of Diseases*, which had been published in London during the preceding year. This book was a document in the Mead-Woodward hostilities.

The remainder of the letter continues the discussion of smallpox which is the unifying theme of most of the correspondence. It illustrates the effort of a contemplative physician to explain observed phenomena. Colden

noticed that smallpox in patients who came from the West Indies tended to assume the fatal confluent form and to be complicated by "Phrensy." He felt that in confluent smallpox the quantity of peccant matter was obviously great and that it should be diminished. To achieve this result the proper method would be purgation. This was dangerous because it was likely to produce stagnation. At this point the reasoning is not altogether clear. Apparently Colden imagined that the physical qualities of the blood rendered it unfit to circulate at normal speed. Since the circulation proceeded slowly and the circulating fluid was of abnormally low specific gravity, stagnation would occur in the head, to which fluids of low specific gravity were attracted. This explained the development of cerebral symptoms in the course of severe smallpox.

When the fever is violent the blood current is so rapid as to prevent the separation of gross components which produce a well developed cutaneous eruption. If the circulation is too slow the peccant parts of the blood cannot be extruded into the skin; any material already extruded returns to the circulation and causes stagnation. In this difficulty the only available method is to strengthen the circulation and to dilute the blood. This may be difficult because the circulation of blood depends on the secretion of spirits; this secretion may be impeded or stopped by the obstruction of arteries which convey blood to the (unnamed) place where secretion occurs.

To the twentieth-century reader Colden's explanations appear to be fully as intricate and nearly as unintelligible as a Javanese ballet. In the effort to explain and rationalize the phenomena of smallpox, Colden used the Harveian circulation upon which he superimposed conceptions of velocity, specific gravity, and sedimentation, which determined the disposition and behavior of peccant pathogens. Part of his chain of thought is missing; it had been detailed in the lost treatise which was entrusted to Halley, and Graham was already familiar with it.

Discussion of the Correspondence

Having examined the content of the individual letters we may now proceed to form an estimate of the correspondence as a whole. The seven letters under discussion, of which five are reproduced herein, afford numerous details of the type that are so necessary for an exact appreciation of the period and of the men who graced it.

Because of his later career in the provincial government of New York, Colden is usually thought of in connection with that province only. The correspondence with Graham depicts him in the earlier role of Phila-

delphia practitioner. The commercial allusion in the first letter as well as many of the earlier documents published in the *Colden Letters and Papers* indicate that although Colden was one of the very few trained physicians at that time practising in Philadelphia, he did not devote all his energies to medicine, but engaged simultaneously in commerce. It would be interesting to learn whether other Philadelphia physicians did likewise at this period.

Of equal interest is the information which the letters provide about Colden himself. An examination of Colden's later life in remote unpopulated or thinly settled parts of New York had led to the surmise that long periods of virtual isolation, as well as the abandonment of a medical for a governmental career, had made it inevitable that Colden's writings in medicine should be speculative rather than observational. A study of the Colden-Graham correspondence makes this supposition untenable, for the letters reveal a marked speculative inclination to have been present at this early stage of Colden's development. We must conclude that Colden was a philosopher at heart and that his abandonment of medical practice probably caused him no severe pang. His subsequent efflorescence as a leading philosopher of colonial America was the maturation of tendencies present early in life.

On almost every page the letters bear witness to the overwhelming importance of the fevers in eighteenth-century nosography. Colden's analysis of the symptoms and their causes has been discussed above. The thinking is predominantly iatromechanical. Hypotheses were fabricated readily and discussed eagerly without being subjected to really strict critique—i. e. to the conscientious collection of crucial data. For example, Colden says much about velocity and specific gravity without ever offering a single measurement or attempting to make one.

Editing of the Text

The manuscripts which are reproduced below, through the courtesy of the New-York Historical Society and its director, Mr. R. W. G. Vail, are in Colden's rather clear handwriting. Punctuation is sparse, capitalization profuse. The letter y (really the Anglo-Saxon letter 'thorn') is used for th. In the following presentation an effort has been made to reprint the text as it appears in the manuscripts, except that superscript letters have been brought down to the line and a very small number of minor alterations have been made, indicated by square brackets. A few conjectural readings are marked by interrogation points.

COPY BOOK OF LETTERS ON SUBJECTS OF PHILOSOPHY MEDECINE OR FRIENDSHIP

Philadia May 25th 1716

For Hugh Graham M. D. To be left wt Mr Hunter

Apothecary in Princes Street London

Sr

You may easily Imagine yt my own interest without ye consideration of our friendship—produced by an airly intimacy betwixt us would engage me to write frequently to you, that I might enduce you to make up to me by writeing ye loss that I doe sustean by my distance from you qt is like for ever to deprive me of the advantages I had in yor conversation. I am only afrayd yt the desire I have of makeing my self acceptable to engage you to continoue a strict correspondence with me may occation my committing a great many Errours ev'n to produce the contrary effects of qt is design'd When we are too anxious to please we become troublsome or Ridicolous I hope you'l pardon my writeing to you at so great a distance such trifling observations as are ye subject of ye following part of this letter I was resolved to write something & I have nothing else. If I finde such as ys pardoned I'l strive to finde something afterwards yt may be acceptable. Things go so well as I cou'd expect since I came last to ys place I hope ye World before too [?] long will see yor merit, reward you accordingly & make you happy ev'n beyond yor wish. You may remember yt I told you if you directed for me at Philadelphia either by vessels directly to ys place or by Vessels or by Vessels to new York or Boston yor letters would come safe to my hands Please to let me know from time to time of any thing that's new in our Bussiness. You may let Mr Innis know that if he'l think it convenient to trust me with any books that you shall recommend to me & with the Philasophicall Transactions as they are published (the last I have is 395) I will take care to remit qt he shall charge by ye first opportunity after I'm assur'd of his sending any thing to me. Pray let me know if Dr Halley has taken any further notice of my Secretions My most humble respects to him Mr Jones Mr Hay & Mr Mach . . .

I am

[no signature]

AN ACCOUNT OF THE FEVER WHICH SEIZ'D 19 OF 26 PERSONS THAT WER ON BOARD THE GLOUCESTER GALLEY OF LONDON DUREING HER PASSAGE FROM THE LANDS-END TO MARYLAND FROM THE 3D OF MARCH, TO THE 10TH OF APRIL 1716

This Fever began as fevers Generally doe with a coldness & shakeing which was succeeded by Violent pains in the Head Back & Bones The pains continued ' till the Fever went off The Pulse was frequent & low They wer from the begining very apt to sweat about ye Face & Shoulders Their Urine was high colour'd without any Sediment frequently it had a white cloud otherwise entirely pellucid If the pain seiz'd the Hind Head the pulse frequently intermitted They wer troubled wt faintings and the Symptoms appear'd more dangerous The Crisis happen'd the 7th 8th or 9th day from the time they wer first taken with ye Chilliness & the disease went off by Sweating

My Design in ye accounts I give is to let you know how far the Theorems I show'd you concerning Fevers wer of use to me in this Fever & that in as few words & as generally as I can. I must only observe by way of preface That I believe if the Gentlemen who make it their chief care to notice the Malignacy of a Fever & who make it their chief intention to oppose that Malignacie by cordials & Diaphoretcs or Sudorifics had they had this under their warm care it would ha' deserv'd the name of Malignant as much as any ever came under their Consideration for when ever our sick endulg'd sweating in the beginning never was any thing more Stubborn than this Fever

You remember the principal propositions of my Theory & the chief are (which I think I demonstrated) That in all Fevers the Blood is thick & exceeds in Quantity. That the chief intention is to lessen the Quantity by Evacuateing the Gross parts & to dilute & attenuate what remains That the greatest difficulty is to know by what methode that Evacuation is to be made. The Rules I gave which likeways noe doubt you remember depended upon the Doctrine of Secretion I delivered to Dr Halley & I am more & more convinced that they depend upon the surest grounds & are the least falacious of any I ever saw delivered in writeing On Generall Corollory I drew was that in all Fevers the sick person was to be keept cool, to hinder as much as possible the most fluid parts being evaporated by the Violence of the Circulation. This was remarkablie confirm'd; for they who had the most care taken of them, wer kept close to their beds, who encuraged sweating, or who were frequently takeing Cordials, every one of them had their Symtoms encreas'd, The Crisis prolong'd to the 11th or 12th day & then imperfect for in 2 or 3 days they all had relapses others again who wer but litle minded, turned out of their beds

& lay in the day time upon the boards, who drank nothing but small beer or Water had the Crisis perfect on the 7th 8th or 9th day

An other Consequence I drew from The Theorems of Secretions and I make it a Rule in my practice Never to Evacuate by any Secretion so long as the Urine continues without Sediment Tho' we had noe Old Woemen yet we had people enough that were bold enough in makeing experiments, & it was easie for every body to observe that all evacuations by purging or sweating at any time where this Rule was not observ'd, encreas'd the Fever & Symptoms & prolong'd the Crisis

My Rule in ys case is to bleed freely according to the fullness, to give plentifully of ye most fluid liquors that doe not increase the Circulation mix'd with some attenuating Medecine, till I observe some sediment in the Urine & then by the Rules in the Essay which I show'd you I know by what Secretion the gross parts are to be evacuated. They who were used after ys manner had the Crisis perfect on the 7th day or sooner without any Relapse

At last you must allow my Vanity to Glory that of these 19 who were all that were sick of the Fever every one of which apply'd themselvs to me first or last, not one dy'd tho' some of them by their own & their friends too great care were brought to the last extremity. I us' very few Medecines the chief Art lyes in the Management & in Catching the Critical Moment & knowing by what Secretion the Crisis is to be made. I am

[no signature]

P: S: One reason that enduced me to communicate this to you was that I suspected this Fever might be a consequent of the hard Winter and that you might have something like it in England, tho' considering the difference of Climates & places we were in from what the people can be with you it is not probable it can be entirely the same.

TO HUGH GRAHAM M. D. Philadia Octr 1716

I wrote to you on the 25th of May last which was not many days after my Arivall at this place But I have not heard once from you since I left England I am the less anxious about it because the letters for this place have been twice left behind by the Carelessness of the Masters of ships. You see I take all the means I can think off to engadge you to keep up a Correspondence with me I wish I may not mistake as I did in the like

Design wt Dr. Douglass. He came from Boston this summer with design to see the Northern parts of America I was glad to see him at Philadelphia & endeavour'd to show how fond I was of renewing our old acquaintance. It soe far pleas'd that he favour'd me with his Thesis from Boston This I thought was a good subject for us to write for both our advantages & I with that freedome which a Comrad might take propos'd some Scruples I had with some things he advanced But to my loss he's soe much displeas'd that he has never since giv'n me an Answear And our Correspondence is nipt in the Bud But I hope you understand me better & I long to hear from you for in a little time I shall become extremely Jealouse

The Small Pox this summer has been Epidemicall. they had not been in the place any time 14 years before & now hardly any escaped I have been as much employ'd as any of my Fellow practitioners & more by the Gentry. I have observ'd my Rules about Fevers answear like Oracles You may think I am full of them & I think I have reasone But without giveing my self any uneasiness upon such considerations I'l relate any thing I observ'd which I think may be any way acceptable to you & I have noe other view in it but that I think what I write may be soe

I allways propos'd in the Beginning Blood letting Some comply'd with it but the most did not I was affray'd to urge because I knew whatever unlucky accedent should happen it would be imputed to any thing they wer averse to. I order'd them to be kept as cool as possible for in the summer time we could not exceed & to give them of any small liquor as much as they enclin'd to take & sometimes more. I gave at Night Diacodium & after the Pustles wer dry'd over the Body purg'd I treated all my Patients after this methode & not one dy'd. The People wer in a litle time convinced of the benefit of ye cool regimen & the hurtfullness of hot things as they thought to drive them out & it became the Generall practice In Woemen generallie the Menses flow'd abat ye time the pox appear'd or a litle before I did not find any prejudice in it Nor in a Flux that sometimes happen'd upon their first appearance I generally let it continue a day without stopping it lessen'd the peccant Matter Soe likewise in the flux kind they generally had severall loose stools upon the turn which prevented ye putrid fever which frequently troubled others When the putrid fever happen'd I cure it exactly according to Sydenh. Methode It is a sure Maxim ye Pox will never fill soe long as the fever continues for the filling is perform'd by a separation of ye gross parts & soe long as ye Motion of ye blood is Violent the parts cannot seperate. I never give Diacodium till ye fever is entirely gone I have had an extra-

ordinary case or two but I must leave that to an other time The Best preservative of ye face is what follows Rx Unguent Dialth ʒij Sperm. Ceti ziij Ol. Tereb z i ss M f. Ung. I have ye effect of this ointment well confirm'd for I know not one it had not sav'd from Pits & yt it is truely ye effect of ye ointment I have ye greater reasone to believe because I know some are pitted all over ye Body except ye face where they us'd ye ointment. But I never use it till ye Pox are dry & yn then anoint morning & night.

The Infection has been soe strong that it has reached all our Dunghill fowls. The pustles are very plainly to be seen about their heads where they want feathers & a great many are found dead of ys desease in the streets

I observe more woemen have dy'd of ys distemper than men

COPY OF PART OF DR GRAHAMS LETTER RECEIV'D THE LAST OF MAY

Febry 1716/17

A young Gentleman about 19 years of Age on the morning of the third day after the Eruption of the small pox was seiz'd with a most violent P[h]renetis that four men could not keep him in bed I was sent for & order a Glyster & blyster to be applyed which had its desir'd effect for that time, all things went well till the 8th day, that I did p[er]ceive his hands to swell, nor turn of a Crimson red colour as they ought to have done, neither did the Pustles plump soe well as they should doe. The 9th day very hot & feverish order'd a Glyster had two stools. Dr Mead was sent for the rather to Satisfy the Patients friends than for any Danger. The 20th day towards the Evening Mead told me that being his hands never swell'd & the pustles never rose he was apprehensive of a 2d fever & therefor advis'd to purge next morning which I agreed to relying on Dr Friends Authority & we ordered Rx Decoct. Sennae Geteonis ʒiij Syr. ros. Solut, zi f. Potio the Poton did not work with him soe he had 10 gr. Pulv. Cornachini & towards the evening had six Stools. That very night & next Morning I perceiv'd all the pustles on his hands & body that were green & full of Matter grow flaccid & collapse these on the face being for the most part dry his face that was the night before swell'd & reddish was now fall'n & some whiter in the Interstices between the Pustles The 12th day the patient was much less feverish & hot & noe bad symptom ensued, he was purg'd two or 3 times more intermitting 3 days & is now Perfectly well

Last Summer I was Physician to a young lady about 27 years of age, who had accustom'd herself for 6 or 7 years to drink very much strong spirits & brandy & noe physic was pleasant to her but strong cordials. She had for several years been troubled with fainting fits sickness at her Stomach & purgeing, but in her last illness of which she dy'd she had these symptoms No Stomach nor Digestion, Continual Vomiting Asthmatic fits Pain of her right side several Icteric symptoms, Continual purgeing of Bilious & porraceous stuff Swelling of her feet Urine sometimes of a fine green colour. And after 3 weeks or a Months illness dy'd Apoplectic I remedied a great many of the most dangerous symptoms but still they recurred. After her Death I obtain'd leave of her friends to open her & from the Desection of the Body you'l find it easy to account for the symptoms

1 The Body very fat & all the fat yellow a great deal of liquid fat was in the Cavity of the abdomen like bile.

2 The Liver was Schirrous & very yellow & of such a Bigness that it fill'd all the upper part of the abdomen & lower part of the Thorax haveing forced up the Diaphragma to near the first rib of the Thorax towards the forepart. It was 23½ Inches long its breadth 11⅔ Inches. It extended from between the first & 2d rib of the Thorax to the last false rib on the right side & to the third false rib on the left side Soe that the Liver was near as much on the left side as on the right & press'd on all the other Viscera of the Thorax & abdomen

1 The Diaphragm was soe much press'd upwards that the lungs had noe more room to play in than the Bigness of an ordinary Mans fist 2 It press'd backwards on the Stomach & right Kidney, on the Oesophagus Aorta & Vena Cava The Other Viscera were not much a Miss, only the right Kidney litle & flaccid. The small room that was left to the Lungs to play in was to me an admiration, the length of the Thorax towards the forepart was scarce an Inch & a half its true they had more room backwards but as it was they were soe much squeez'd, that I have frequently wonder'd the asthmatic fits were not more Violent

---

PART OF A LETTER I SENT TO DR HUGH GRAHAM

June 1717

It is with great pleasure I find my self agree in the same thoughts with a man of Dr Friends Character. I was convinced that purging in the small Pox is necessary as soon as the swelling of the face falls if the

swelling be not supplied by that of the hands soe far as to prevent a fever. For the Gross parts which stagnated in the face doe again mix with the blood, make it thick & produce a fever, they should therefor be evacuated & that by purgeing because they seperate best by that Secretion. I am confirm'd in this reasoning by observeing that the sick have frequently several stools upon the turn very much to their advantage.

Last year & soe much of this as is past have been more rainy than common & have produced frequent deseases of the head & nerves that is Coma Convulsions & Palsies. The parts of the blood which are carried to the Head are lighter than what is distributed by the great Artery & in a light air the parts of the blood are less solid or lighter therefor in such a constitution the Head is more likely to be effected. But because the light parts are best evacuated by stool I found purgeing allways remov'd these distempers. I us'd such as were compos'd of Scammony & Colocynth. I found indeed a near Relation betwixt the Deseases of the Belly & the head. The Colic pains frequently ended in Convulsions & Palsies these that had their nerves affected were more than common lyable to the Colic These pains of the Belly were cur'd likewise with the greatest certainty by purgeing. The Intermitting fevers too this yeare were accompanied with or ended in these violent Colic pains & palsies The Bark was soe far from cureing these Belly achs that it seem'd rather to cause them But these likewise were prevented by purgeing before giveing of the Bark & if purgeing did not proceed, the fevers of this Constitution did not yield to the bark but return'd with some odd symtoms.

I am soe fond of my own Productions as to believe that my theory pointed to me these Methods of cure by which I have been very luckie in my practice. I hope to have my thoughts before long in such order as they may be laid before you for your Opinion—You see I doe to the utmost of my poor Abilities to engadge to engadge [sic] your Correspondence I have thought it better to give these general Rules which I have confirm'd by particular observations, than to trouble you with the particular Historys which probablie might cost you more than they are worth. I forgot to tell you that the use of hot things in the Colics I above mention'd certainly brought on Convulsions & at last palsies or the loss of the Limbs.

To Mr: Hugh Graham Doctor of Medicen

Dr Sr:

I have your kinde Letter of the 15th of June Last which I received only 3 days agoe I received Dr. Woodwards book with some others from Mr Innys I opened it full of hopes of receiving a great deall of Instruction & Pleasure from a book on the subject from a Proffesor butt I believe I need nott tell you how miserably I was disappointed every was for I finde he has meant nothing Less than the giving us a true state of Physic & has started an Hypothesis which can produce nothing butt surprise. I am concerned a Physician so eminent as Dr: Mead has by his Pasion given so much ground for reproach. the Pamphlett published against Dr: Woodward such as I have seen discover chiefly the mean opinion thy have of him butt an enemy never has more advantage then when he is dispised for my part I think his book required noe answer for it must be unnecessary to them that understand the animal Oeconomy & the others can only be led by their Prejudices & opinions of the men. Being upon his book putts me in minde of what I once wrote to you that we finde the small pox here very fatall to those that come from the west Indies which generally have ye Confluent kinde & dye after or about the 14th day [;] the small pox never rises as well upon them as others nor does their face & hands swell sufficently after their face falls & the pustles [?] dry their pulls becomes frequent indeed butt continues very Low & they become Phrentic & they seem nott to dye of a fever as of a Phrensy an old Practioner here tryed the Method of Purging on two or three of them where I was a witness & without any success I am sure he did not under doe itt; this has made mee think thatt tho' the quantity of Peccant Matter in the confluent kinde plainly enough Indicates the nesesity of Diminishing & the only Secretion that can be done by be by Purging yett in some cases the Circulation soe far flags that they can bear noe evacuation without indangering a Stagnation for the parts which the blood then abounds is very unfitt to continue its motion & I believe when the blood moves slowly with specifically light gross parts it is aptest to stagnate in the head or about the brain I am the more convinced of this from a child a Barbadian about 10 or 12 years of age I saw the day before she died that had been treated according to the cooll Regimen the small pox never rose well as they told mee & her face nor hands had swelled very little this child was free of any pain had her sences perfect butt her pulls could hardly be felt she dyed by the circullations gradually Dimenishing with-

out any remarkable symtom I am far from writting this to descry the cool Regimen as it is called butt to instance how dangerous the errors are in exceeding in itt & useing itt universally without exception While the fever is violent the gross parts can never separate & form the pustles butt if the Circulation flag the peccant parts (to make the word more generall) cannott be forced out into the skin nor can the face nor extremities swell and if any thing before has been seperated it has room to return within the Area of Circulation & then the likeliest as well as most dangerous place for it to stagnate is in the Membranes of the brain by which it comonly causes a Phrensy or sudden convulsions how these symptoms can be removed when the circulation is theis slow I must confess to you I cannott tell for their is no room for Revulsions or Derivations or Lessining the Peccant matter by any Evaccuations the only method left I think is to strenthen the Circulation & to dilute the blood in hopes the Circulation may att last remove the stagnation butt how small must our hope be when the secretion of spirits without which the Circulation cannot be carried on is impeded & may be intirely stopt by the stagnation in the arterys which carry the blood to the place where the secretion of spirits is made the fever which comes after the face drys & swells I am convinced is caused by the matter which stagnated in the face & caused its reentering the Circulation & theirfor ought to be evacuated & I believe most properly by stooll butt then wee must take speciall care that the circulation doe not flag the Evacuations & medicens which advance the circulation such as cordials & alexipharmais both lessen the most fluid parts & theirfor I believe nott less necesary to Dilute the blood what I propose to myself in the cure of the small pox is to keep the circulation in such a state that itt may nott hinder the seperation of the gross parts by being to swift[,] that itt be not soe weak as nott to be sufficient to drive them out into pustles when they doe seperate or to allow them to seperate soe near the heart as to indanger a stagnation in the more noble Viscera When I can have noe more hopes of the peccant matters being seperated by pustles through the skin, then to carry them off by a more proper secretion & Lastly when the fluids of the blood & spirits are wasted by a long sickness & fever to take all the precautions that the Circulation doe not flag, I write this to you as my friend that I may have your Judgment freely upon these rules I have laid downe to my self It is the duty of Physicians who deal in nothing Less then Health or sickness hapiness or Misery Life or death to be very forward not only to ask advice butt to give itt I am the more concerned to know your opinion of them because they are only generall Corrolarys from a more generall Theory of fevers sketches of which I

communicate to you & of which I must own I am very fond if you entertain a good opinion of what I write I must beg the favor of your discorsing with your acquaintances upon itt & lett me know wether itt doe nott seem a rationall as well as plain Method to goe by in this disease & then perhaps I shall have the honnour to Communicate some more generall rules by which I apply these thoughts more Universally to all fevers I designed when I began to have entertained you with a thought in naturall Philosophy which Perhaps might had been more amusing butt I have tun soe far beyond the bonds I had sett to my self when I began this Letter that I must Leave itt to the next oppertunity of writing I am very much concerned to hear of your Late Ilness & especially about the symptoms of your Lungs for gods sake dont dally with your distemper my Wife is in very good health & pretty big about the waste wee wish you all hapiness with yours

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A BIBLIOGRAPHY OF INTERNAL MEDICINE: YELLOW FEVER *

ARTHUR L. BLOOMFIELD **

It seems natural that so dramatic and picturesque a disease as yellow fever, and one for a long time so puzzling, should have been much written about. The history of the subject is well covered in Henry Rose Carter's scholarly book, *Yellow Fever* (Baltimore, The Williams and Wilkins Company, 1931) to which is appended an extensive bibliography. George Augustin's huge volume (*History of Yellow Fever.* New Orleans, Searcy and Pfaff, Ltd., 1909) also contains much interesting, though undocumented, material. Extensive historical bibliographies are to be found in the works of La Roche (Ref. 3) and of Hirsch (Ref. 5) and the recent literature is well covered in Strode's monograph (Ref. 29). There is an immense number of books dealing, often dramatically, with individual epidemics, in some of which are listed in ghoulish detail the names of the many hundreds who died of the disease (Keating, J. M.: *The Yellow Fever Epidemic of 1878 in Memphis, Tennessee.* Printed for the Howard Association, 1879). Of special interest is the book edited by Chuncey D. Leake, *Yellow Fever in Galveston, Republic of Texas, 1839* (Austin, University of Texas Press, 1951) which contains admirable brief "Stories of the men who conquered yellow fever," and finally there is a readable account of the whole subject in H. Harold Scott, *A History of Tropical Medicine* (Baltimore, Williams and Wilkins, 1939), Vol. I, Ch. VII, yellow fever, p. 279.

Yellow Fever

Bacteriology—Refs. 7, 14.
Clinical—Refs. 1, 2, 3, 4, 21.

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* From the Lane Medical Library.

** Supported by a grant from the California Foundation for Medical Research. The considerations that led the author to engage upon a large scale project of a bibliography of internal medicine have been outlined by him in the prefaces to two earlier installments, viz. on typhoid fever (*Bulletin of the Johns Hopkins Hospital*, 1932, *51*: 234-258) and scarlet fever (*Stanford Medical Bulletin*, 1952, *10*: 114-129). To quote from the latter: "It was our thought, twenty years ago, that a selected bibliography—a *bibliographie raisonnée*—which listed the important contributions of modern medicine (nineteenth and twentieth centuries) with critical comments, might be useful and might stimulate both students and doctors to further reading."

Epidemiology—Refs. 5, 18, 17.
General—Refs. 1, 2, 3, 4, 26, 29.
Jungle yellow fever—Ref. 13.
Mosquito and yellow fever—Refs. 6, 9, 10, 16, 26.
Pathology—Refs. 2, 12, 18.
Transmission to man—Resf. 9, 10.
Transmission to monkeys—Refs. 11, 15.
Vaccination against yellow fever—Refs. 23, 27.
Virus of yellow fever—Refs. 19, 24, 25, 26, 28.
Yellow fever in mice—Refs. 20, 22.

1. Rush, Benjamin: *An Account of the Bilious Remitting Yellow Fever as It Appeared in the City of Philadelphia in the Year 1793.* Philadelphia, Thomas Dobson, 1794, Second Ed.

We may well open this bibliography with an account of yellow fever by one of the outstanding physicians of the time. However, Rush's thinking and description are definitely oriented to the 18th Century and actually are of little value except as historical evidence. " The causes which induced indirect debility were, 1. Fatigue of body or mind induced by labour. . . . It was labour which excited the disease so universally among the lower class of people. . . . A hard trotting horse brought it on two of my patients. . . . A fall excited it in a girl. . . . 2. Heat from every cause but more especially heat of the sun was a very common exciting cause of the disorder. . . ." In addition intemperance, fear, grief, cold, etc. were assigned as precipitating causes (p. 29 et seq.). The clinical description is based on the humoral theory of disease: " The blood vessels are the ' seat and throne ' of this as well as of all other fevers. I have publickly taught for several years, that a fever is occasioned by a convulsion in the arterial system " (p. 40). Rush goes on to describe in great detail the pulse and other features of the disease without of course any adequate physical examination. " There were for several weeks two sources of infection viz. exhalation and contagion. The exhalation infected at a distance of three and four hundred yards; while the contagion infected only across the streets " (p. 104). As to the causes which checked the disorder: " On the fifteenth of October it pleased God to alter the state of the air. The clouds at last dropped health in showers of rain. . . . The appearance of this rain was like a dove with an olive branch in its mouth, to the whole city " (p. 130). The discussion of treatment is fanciful and need not be described in detail (p. 193 et seq.). There are elaborate and

controversial discussions of the place in therapy of purging, blood-letting, blisters, etc. Rush came to feel the disease was not contagious and in his *Medical Inquiries and Observations* (Philadelphia, Benjamin and Thomas Keith, etc., 1809, Third ed., Vol. 4, pp. 235-284) he has a long section giving his arguments in detail. Of course the truth of the matter is that yellow fever *is not* contagious although rapidly transmitted by the mosquito about which Rush knew nothing at the time.

Rush was undoubtedly a great man and a great physician in his day; but we must think of him as an exponent of 18th century medicine and not of the new views developed in France at the turn of the century.

A graphic account of this epidemic is given by W. S. Middleton ("The yellow fever epidemic of 1793 in Philadelphia." *Annals Med. Hist., 10*: 434, 1928), and by J. H. Powell. (*Bring Out Your Dead. The Great Plague of Yellow Fever in Philadelphia in 1793.* Philadelphia, University of Pennsylvania Press, 1949). With regard to Rush and his ideas on yellow fever see C. E. A. Winslow: *The Conquest of Epidemic Disease.* Princeton, 1943, Ch. XI, The enigma of yellow fever (p. 193).

2. Louis, P. Ch. A.: *Anatomical, Pathological and Therapeutic Researches on the Yellow Fever of Gibraltar of 1828.* Translated from the manuscript by G. C. Shattuck, Jr., M. D. Boston, Charles C. Little and James Brown, 1839. [Precedes the French Edition.]

Once more, as in the case of his book on typhoid fever, one must pay homage to Louis for his cool objectivity, the handling of facts, and his clear analysis of results. Louis was of course an outstanding member of the young group of French clinician-pathologists at the beginning of the 19th century who revolutionized and modernized the study of disease. Louis with Chervin and Trousseau were sent as a commission by the French government to Gibraltar to study the epidemic of yellow fever then rampant. Louis set out to do autopsies on fatal cases with meticulous care; these were correlated with objective clinical findings. The descriptions are masterpieces of clarity and precision and would do credit to a modern medical service or autopsy room. Anatomically Louis soon centered his attention on the liver. "But the most remarkable lesion of the liver was the alteration of its color. . . . This alteration consisted in a discoloration, the liver being sometimes the color of fresh butter, sometimes of a straw color, sometimes of the color of coffee and milk, . . ." etc. (p. 117). Later in interpreting the lesions: "Lesions, apparently slight,

may explain death when they have taken place rapidly, as in the cases now under consideration. And then again, in the present state of science we can not appreciate the nature of the specific lesion of the liver, and consequently we can not determine how far it had anything to do with the death" (p. 144). "As then, a strict analysis . . . proves the existence of a cause, unequal in its operation, and of which but one effect is constant, the specific alteration of the liver, and as in a third part of the cases it is directly to this cause that we are obliged to refer the death, we naturally ask how does this cause act, through the medium of what system does it exert its influence on the economy?" (p. 164). But the objective Louis speculates no further; he simply assembles evidence and waits. He shows later by extensive statistics that yellow fever confers an immunity to second attacks even after a lapse of many years. As to treatment he compares that of the English and the Spanish physicians at Gibraltar. "It is true, that, in the army, the ratio of mortality was one in four and a half, and that, in the city, it was only one in six, so that, all other things being equal, it would seem to result from these two principal classes of facts, either that the treatment of the Spanish physicians was very efficacious, or that of the English physicians was very injurious" (p. 330).

The translator in his introduction says this about Louis: "Some seem to have been misled by the term numerical system, which has been said to be that of M. Louis. They seem to have thought that his peculiarity consists in this merely, that he counts. . . . We call some experienced, scientific. Is it not by comparing individual cases, by adding what they have observed in one to what they have observed in another, by *counting*, that they have become so?" (p. xii).

3. La Roche, R.: *Yellow Fever, Considered in Its Historical, Pathological, Etiological, and Therapeutical Relations.* Philadelphia, Blanchard and Lea, 2 Vols., 1855.

This monumental treatise deals with every phase of the subject which is made vivid by innumerable allusions to the literature and to the opinions and actions of individual physicians as well as commissions, boards of health, etc. It opens with a bibliography of over a thousand titles, presumably definitive to date. The next section of some 100 pages deals with the various Philadelphia outbreaks. Symptoms and clinical features are then discussed in minute detail (pp. 129-383), after which comes a chapter on pathological anatomy of the disease. As to the liver "The secreting cells were pale, ill-defined, and less granular than when in the

normal state. In the cells with few exceptions, no nucleus could be detected. . . ." The incubation period was regarded for the most part as from three to six days. " Dr. Harrison, of New Orleans, remarks, that persons who arrive in that city during an epidemic, from the healthiest regions . . . are subject to attack on the sixth, fifth, fourth, or even as early as the third day after their arrival," a finding which indicates the promptness with which people are likely to be bitten by mosquitoes in an infected area. There is next an analysis of the mortality of yellow fever (pp. 513-546).

Volume II deals largely with the questions of etiology and contagion. There was of course no idea of a living virus or of any part played by mosquitoes. Among various " circumfusa " which we now know have nothing to do with the disease, such as light, electricity, and atmospheric pressure, the writer was quite clear as to the relation to temperature. " The yellow fever is undeniably a disease of hot climates and hot seasons." As to the immediate cause " much diversity of opinion has existed . . . relative to the nature of the efficient cause of yellow fever—to the sources whence that cause is derived—as well as to the mode of diffusion of the disease after it has once made its appearance. By one set of physicians . . . the disease is regarded as the offspring of a morbid poison found and elaborated in a diseased body, and is invariably referred to importation from abroad." By others " the fever has been invariably referred to the evolution, under particular circumstances, both terrestrial and meteorological, of a febrile poison originating in molecular changes . . . independent of . . . the arrival of vessels from sickly ports " (Vol. II, p. 190). The major portion of this volume is then devoted to an intensive discussion of contagion versus non-contagion, to the latter of which the author adheres. He tells how important physicians have changed from contagionists to non-contagionists. " Dr. Rush, who, as we have seen, had originally espoused the cause of contagion . . . at last relinquished his former views. Not satisfied with doing so silently . . . this eminent physician published a formal recantation. . . ." [1] (Vol. II, p. 253). However there was a common belief that the disease arose by " infection " with a " miasma " and interestingly enough many analogies with malaria were

[1] " To prevent recurrence of the fever, I early pointed out the domestic origin. In this opinion I was opposed by nearly the whole College of Physicians, who derived it from a foreign country, and who believed it to be a specific disease. They were followed by nearly all the citizens of Philadelphia."—*The Autobiography of Benjamin Rush*, ed. George W. Corner. Princeton, 1948, p. 97.

The whole question of anticontagionism is thoroughly discussed in Erwin H. Ackerknecht's Anticontagionism between 1821 and 1867, *Bull. Hist. Med.* 1948, *22*: 562.

noted. " It is, like other miasmal diseases, under the influence of atmospheric vicissitudes. . . ." " It is limited in the sphere of its prevalence, never extending beyond certain boundaries, attacking those who venture within those boundaries, even in the absence of the sick, and sparing those who keep aloof. To these limits it may be restricted by a cessation of intercourse; while the disease is arrested in its progress by the dispersion of the inhabitants of the infected localities, and by preventing further access to these by artificial means " (p. 595). Under the subject of prophylaxis " it is necessary to effect the removal of all local nuisances before the accession of hot weather . . . whereas, docks or other localities liable to be visited by the disease, courts, yards, gutters, cellars should be left perfectly clear; all accumulations of filth and stagnant water should be carefully removed; streets, courts, and alleys should be paved." The holds of vessels must be left clean and pure, and all foul bilge water carefully pumped out. . . ." (Vol. II, p. 732). The author, however, opposed quarantine both because he thought it useless and because of inconvenience to individuals and commercial losses (Vol. II, p. 737). " Much more useful will it be to obtain at once the complete evacuation of all the inhabitants of infected spots " (Vol. II, p. 749). Under treatment a fantastic list of agents is discussed—bleeding, local depletion, emetics, purgatives, mercury, diaphoretics, sedatives, oxide of bismuth, rhatamy, tannin, creosote, adrue, nitrate of silver, lime-water, chloroform, opium, external applications, affusions, drinks, injections, cool air, sinapisms, moxa, actual cautery, Peruvian bark, sulphate of quinia, oil, melambo, charcoal, moral treatment, and diet.

4. Macdonald, J. Dennis: " Yellow fever." *A System of Medicine*, ed. J. Russel Reynolds. London, 1870, Second Ed., Vol. I, p. 475.

A standard account of the disease shortly before the bacteriological era shows little recent advance in knowledge. The author states that the infectious nature of yellow fever is now generally admitted. He is unclear however as to how the disease is transmitted and goes definitely wrong when he says that " the clothing of infected persons or of healthy persons having communicated with infected places or persons may impart infection to other places or persons." Under treatment are mentioned the usual congeries of drugs. " Acetate of ammonia, nitrate of potash, nitrous ether, and tincture of squills and henbane may be combined with lime-juice for a drink."

An authoritative modern discussion of the clinical aspects and diagnosis

of yellow fever is that of J. Austin Kerr in Strode (Ref. 29, p. 381 *et seq.*). In this article the findings of various clinical laboratory tests are summarized, as well as the laboratory tests for diagnosis, and treatment.

5. Hirsch, August: *Handbook of Geographical and Historical Pathology* [Trans. from the second German edition]. London, The New Sydenham Society, 1883, Vol. I, p. 316, Ch. VIII, Yellow fever.

This is the great storehouse of information about epidemics of yellow fever which are listed in detail as to time and place. It is pointed out that yellow fever rarely occurs as a pandemic and that epidemic outbreaks never happen suddenly; a series of isolated cases always precedes them. Often the disease remains limited to one quarter of a town. The duration of epidemics varies greatly. Many other circumstances of outbreaks are discussed. Hirsch regards it as shown that yellow fever is communicable; "it is not now a question of whether communication actually takes place but only of how it takes place." Differences of opinion on this point "depends not so much upon conflicting facts, but rather the significance ascribed to them." There is of course no mention of the mosquito.

The article is followed by a lengthy bibliography.

6. Finlay, Carlos J.: "The mosquito hypothetically considered as the agent of transmission of yellow fever." Translated by Dr. Finlay from the *Anales de la Academia de Ciencias Médicas, Físicas y Naturales de la Habana*, 1881, Vol. 18, p. 147. In *Trabajos Selectos del Dr. Carlos J. Finlay*. Habana, Republica de Cuba, 1912.

To Finlay seems to belong the credit for first clearly insisting that the mosquito was the vehicle for transmission of yellow fever.[2] "I feel convinced that any theory which attributes the origin and the propagation of yellow fever to atmospheric influences, to miasmatic or meteorological conditions, to filth or to neglect of general hygienic precautions, must be

[2] The long rambling article by Josiah C. Nott ("Yellow fever contrasted with bilious fever—Reasons for believing it a disease sui generis—Its mode of propagation—Remote cause. Probable insect or animalcular origin," etc. *New Orleans M. and S. J.*, *14*: 563, 1848) to whom credit is often given for first claiming that yellow fever is transmitted by the mosquito is thus criticized by Ronald Ross (*Memoirs*, London, John Murray, 1923): "Speculations of this kind are apt to be much overrated by penmen. It is easy for persons to sit in arm-chairs and weave hypotheses; Many imagined America before Columbus; but an ocean had to be traversed between the dream and reality. Theorists who do not trouble to verify their own speculations deserve little credit . . ." (p. 424).

considered as utterly indefensible." "I shall not concern myself with the nature or form of the morbific cause of yellow fever, beyond postulating the existence of a material, transportable substance, which may be an amorphous virus, a vegetable or animal germ, a bacterium, etc., but at any rate constitutes something tangible which requires to be conveyed from the sick to the healthy before the disease can be propagated." He then enumerates the reasons which made him select the mosquito as the only suitable vector. Finlay postulated that the mosquito carried the virus from one person to another on the mouth-parts. He made five attempted inoculations with mosquito bites with equivocal results. He finally concludes: "Should it finally be proven that the mosquito-inoculation not only reproduces the yellow fever, but that it constitutes the regular process through which the disease is propagated, the conditions of existence and of development for that dipterous insect would account for the anomalies hitherto observed in the propagation of yellow fever. . . ."

Apparently Finlay did not stimulate anyone to further investigation at the time, and of course his idea that the virus was mechanically transmitted was incorrect; had he known that the infective period of yellow fever lasts for only three or four days and that the mosquito must carry the virus for about twelve days before its bite becomes dangerous, he might have actually solved the problem. Nonetheless he deserves full credit for pushing the idea of mosquito transmission. Later Finlay ("Yellow fever. Its transmission by means of the Culex mosquito." *Am. J. M. Sc.*, *92*: 394, 1886) described in interesting fashion the feeding habits of local mosquitoes and reported more transmission experiments which again did not seem to convince anyone. But Finlay continued to expose his views as for example in a systematic discussion ("Yellow fever." *Edinburgh M. J.*, *40*: 35, 1894) and in many papers, some of which are controversial, reprinted in Selected Papers.

Finlay's life and work are sympathetically appraised by Carlos E. Finlay (*Carlos Finlay and Yellow Fever*. New York, Oxford Univ. Press, 1940).

7. Sanarelli, J.: "Étiologie et pathogénie de la fièvre jaune." *Ann. Inst. Pasteur*, *11*: 433, 673, 1897.

With the development of the bacteriological era many physicians became convinced that yellow fever was caused by a living germ. As was the case with almost all infectious diseases reports poured in of claims for one or another bacterial agent. So it was with yellow fever; the early

claims are concisely summarized in *Carlos Finlay and Yellow Fever* (Ref. 6) only to be demolished by George M. Sternberg (*Report on the Etiology and Prevention of Yellow Fever.* Washington, Government Printing Office, 1890) who states in his conclusions: "The specific infectious agent in yellow fever has not been demonstrated. The most approved bacteriological methods fail to demonstrate the constant presence of any particular microorganism in the blood and tissues of yellow fever cadavers" (p. 221). However, Sanarelli's long paper was so emphatic with its report of isolating a specific bacterium, bacillus icteroides, both from living patients and at autopsy, and the reproduction of lesions compatible with those of yellow fever in small animals that many were convinced for some years. Sodré and Couto for example in their authoritative treatise on yellow fever (*Das Gelbfieber.* Wien, Alfred Hölder, 1901) accept the organism of Sanarelli without reservation (p. 63 *et seq.*). An abbreviated translation of Sanarelli's paper also appeared in English ("A lecture on yellow fever." *Brit. M. J., 2*: 7, 1897). But soon thereafter Reed and Carroll of the Army undertook to study B. icteroides. Reed was particularly well qualified, having worked on hog-cholera with Welch at Johns Hopkins. In their first paper (Reed, Walter and Carroll, James: "Bacillus icteroides and Bacillus cholerae suis. A preliminary note." *Med. News, 74*: 513, 1899) they gave cultural and experimental data for considering Sanarelli's organism as simply a variety of the hog-cholera bacillus. Sanarelli unfortunately took this report as a personal affront and in another paper struck back (Sanarelli, G.: "Some observations and controversial remarks on the specific cause of yellow fever." *Ibid., 75*: 193, 1899) angrily but feebly. To this Reed and Carroll replied once more ("The specific cause of yellow fever. A reply to Dr. Sanarelli." *Ibid.*, p. 321), "We pass by, therefore, as unworthy of comment, Sanarelli's insinuation that we could lend ourselves to the support of any personal controversy. . . ." They gave many more data and arguments and finally in a very comprehensive report ("A comparative study of the biological characters and pathogenesis of Bacillus X (Sternberg), Bacillus icteroides (Sanarelli) and the hog-cholera bacillus (Salmon and Smith)." *J. Exper. Med., 5*: 215, 1900) laid the ghost of this controversy.

8. Carter, H. R.: "A note on the interval between infecting and secondary cases of yellow fever from the records of the yellow fever at Orwood and Taylor, Miss. in 1898." *New Orleans M. and S. J.*, *52*: 617, 1900.

That brilliant, if eccentric, student of yellow fever, H. R. Carter, made the important observation (in a rural community where circumstances were such as to make the induction valid) that after the first case of yellow fever appeared in a certain locale at least two weeks elapsed before secondary cases arose. Although Carter drew no (written) conclusions as to insect carriage his work helped to strengthen in Walter Reed's mind the probability that the mosquito was the vector of yellow fever (Ref. 9).

9. Reed, Walter, Carroll, James, Agramonte, A., and Lazear, Jesse W.: "The etiology of yellow fever. A preliminary note." *Philadelphia M. J.*, *6*: 790, 1901.

The famous yellow fever commission consisting of Walter Reed,[3] James Carroll, Jesse W. Lazear and Aristides Agramonte had recently been assembled at Havana where yellow fever had appeared in American troops. This is the first of a series of communications announcing the work of the Commission and its conclusions. This report deals first with cultures from yellow fever cadavers with more comments on B. icteroides. (See Ref. 7.) The second part gives a review of Carter's work (Ref. 8). "In view of the foregoing observations we concluded to test the theory of Finlay (Ref. 6) on human beings." Non-immune men were bitten by infected mosquitoes (Culex fasciatus). "It will be seen that we record nine negative and two positive results." The two positive cases were bitten by mosquitoes which had fed on yellow fever patients 12 and 10 days previously; the "failures" were bitten by mosquitoes which had fed 2-8 days previously. The two cases reported in detail are Carroll, who survived and Lazear who died.

The final conclusion of the paper is: "The mosquito serves as the intermediate host for the parasite of yellow fever, and it is highly probable that the disease is only propagated through the bite of this insect."

In the second communication (Reed, Walter, Carroll, Jas., and Agra-

[3] By far the best and most accurate account of the yellow fever commission and its work is that of Howard A. Kelly (*Walter Reed and Yellow Fever*. New York, McClure, Phillips and Co., 1906). There is a full bibliography of the papers written by the members. Other books on Walter Reed dealing to some extent with yellow fever are Albert E. Truby: *Memoir of Walter Reed*. New York, Paul B. Hoeber, 1943; and L. N. Wood: *Walter Reed, Doctor in Uniform*. New York, Julian Messner, Inc., 1943.

monte, Aristides: "The etiology of yellow fever. An additional note." *J. A. M. A.*, *36*: 431, 1901) the authors tell first of the establishment of an experimental sanitary station (hospital) under full quarantine for the continuation of their studies. There are then reported the details of further inoculation experiments from which the following important conclusions are drawn: An interval of about 12 days or more after contamination appears necessary before the mosquito is capable of conveying the infection. The bite of the mosquito at an earlier period does not appear to convey any immunity. Yellow fever can be produced by subcutaneous injection of blood drawn during the first and second days of the disease. The incubation period of the experimental infection varied from forty-one hours to five days and seventeen hours. Yellow fever is not conveyed by fomites. The spread of yellow fever can be most effectively controlled by destroying mosquitoes and protecting susceptible people against mosquito bite. Thus the principal facts of the mode of transmission of yellow fever were definitely established. In a third note (Reed, Walter, Carroll, James and Agramonte, A.: "Experimental yellow fever." *Am. Med.*, *2*: 15, 1901), they describe in detail the clinical features of the experimental disease and emphasize the occurrence of mild cases which can only be diagnosed with difficulty. Another paper (Reed, Walter, and Carroll, James: "The prevention of yellow fever." *Med. Rec.*, *60*: 641, 1901) soon followed in which the harmful mosquito is described with its habits of breeding and feeding and a review of preventive measures including quarantine which they found requires a longer period than the usual five days. Meanwhile Reed gave an interesting review of all his work (Reed, Walter: "The propagation of yellow fever; observations based on recent researches." *Ibid.*, p. 201) before the Medical and Chirurgical Faculty of the State of Maryland. Finally in the last of Reed's early series of papers (Reed, Walter, and Carroll, James: "The etiology of yellow fever. A supplemental note." *Am. Med.*, *3*: 301, 1902) he reports producing yellow fever with serum which has been passed through "a new Berkefeld laboratory-filter." He discusses the possibility of filterable toxin, but concludes that the agent is probably a filterable virus.

10. Reed, Walter: "Recent researches concerning the etiology, propagation and prevention of yellow fever, by the United States Army commission." *J. Hyg.*, *2*: 101, 1902.

The fruits of the Commission's studies were not only an understanding of the transmission of yellow fever but invaluable clues to control of the

disease. In this lecture Reed after reviewing material already presented in his previous papers tells how under the efficient managment of General Gorgas yellow fever was practically extirpated in Havana in one year. In October 1900, for example, the epidemic reached a peak of over 300 cases; in the same month a year later there were only three cases. This amazing result was achieved by quarantine, screening, and mosquito destruction, and set the precedent for the final elimination of the disease in civilized communities, as epidemic after epidemic was stamped out and endemic foci were cleared. Gorgas himself (Gorgas, William C.: "Sanitation of the tropics with special reference to malaria and yellow fever." *J. A. M. A.*, *52*: 1075, 1909) summarized his methods and accomplishments.

A recent authoritative book on all aspects of mosquito control is that of William Brodbeck Herms and Harold Farnsworth Gray (*Mosquito Control*. New York, The Commonwealth Foundation, 1944, second ed.).

11. Thomas, H. Wolferston: "Yellow fever in the chimpanzee." *Brit. M. J.*, *1*: 138, 1907.

There was a general belief that man was the only intermediate host of yellow fever; in other words the cycle was thought to be mosquito—man—mosquito. Thomas was the first to report the infection of a monkey by mosquito bite. A large number of mosquitoes were fed on a yellow fever patient, and 21 days later the survivors were allowed to bite a chimpanzee who 27 hours later developed fever and ran the clinical course of yellow fever. This experiment anticipated the classical studies of Stokes, Bauer, and Hudson (Ref. 15) and also raised the important question of whether animals aside from man could be storehouses of yellow fever virus in nature (Jungle Fever, Ref. 13). The whole question of various mammalian hosts of yellow fever is discussed in Strode (Ref. 29, p. 303 et seq.).

12. Da Rocha-Lima, H.: "Zur pathologischen Anatomie des Gelbfiebers." *Verhandl. d. deutsch. path. Gesellsch.*, *15*: 163, 1912.

Da Rocha-Lima was one of the first to devote a paper exclusively to the anatomical changes of yellow fever, which are gone into in detail. However, as early as 1890 W. T. Councilman at the instigation of Sternberg made a report on the histology of yellow fever with special emphasis on the liver changes (*see* Sternberg, Geo. M.: *Report on the Etiology and Prevention of Yellow Fever*. Washington, Government Printing Office, 1890, p. 151), and E. Marchoux and P.-L. Simond ("Études sur la fièvre

jaune." *Ann. Inst. Pasteur, 20*: 161, 1906) in the course of their elaborate studies of yellow fever have a section on the pathology of the disease with numerous colored plates. The early studies of Louis (Ref. 2) are also not to be forgotten. For a recent critical discussion of the lesions with bibliography see Strode (Ref. 29, p. 141 et seq.).

13. Balfour, Andrew: "The wild monkey as a reservoir for the virus of yellow fever." *Lancet, 1*: 1176, 1914.

Balfour raised the question of whether wild monkeys might not act in some places as a reservoir of yellow fever on the basis of a story that in a certain part of Trinidad red howler monkeys are said to be found dead and dying in the forest preceding an epidemic of yellow fever. Certain people who went into the woods also acquired yellow fever. Various outbreaks of yellow fever such as that reported by F. L. Soper, H. Penna, E. Cardoso, J. Serafim Jr., M. Frobisher Jr., and J. Rinheiro ("Yellow fever without Aedes aegypti. Study of a rural epidemic in the Valle do Chandan, Espirito Santo, Brazil, 1932." *Am. J. Hyg., 18*: 555, 1933) were obviously not caused by bite of Aedes aegypti and did not occur on the basis of the classical man—mosquito—man cycle. Studies of this sort led to the concept of jungle yellow fever with a forest or animal—mosquito cycle propagated in remote areas into which man steps only secondarily. It is only in such areas that yellow fever is today endemic. The problem of jungle fever and localities in which foci persist is described in detail in Strode (Ref. 29, p. 463 et seq.).

14. Noguchi, Hideyo: "Etiology of yellow fever. I. Symptomatology and pathological findings of the yellow fever present in Guayaquil." *J. Exper. Med., 29*: 547, 1919.

The formation of the Rockefeller Foundation Yellow Fever Commission is told in Strode's book (Ref. 29, p. 14 *et seq.*). Noguchi was a member of the Commission and in the above paper and its sequels he announced the cause of yellow fever as a leptospira which he named "Leptospira icteroides." This claim of course turned out to be erroneous, and Noguchi's apologists have stated that he was actually working on Weil's disease which is easily confused with yellow fever and which is in fact caused by a leptospira. A careful scrutiny of the original articles, however, throws doubt on this position. In the present paper, for example, Noguchi describes at length the disease upon which he worked in the Yellow Fever Hospital at Guayaquil; he points out the skill and experience of the per-

sonnel and he lays special stress on the accuracy of the diagnosis. In the second paper ("II. Transmission experiments on yellow fever." *Ibid.*, p. 565) he injected blood from yellow fever patients into many varieties of animals and thought he produced a disease analogous to yellow fever in guinea pigs. "In the blood, liver and kidneys of guinea pigs experimentally infected with the blood of yellow fever patients a minute organism was demonstrated. . . . The leptospira obtained from cases of yellow fever has been given the provisional name of Leptospira icteroides." In the next paper ("III. Symptomatology and pathological findings in animals experimentally infected." *Ibid.*, p. 585) the experimental disease produced in guinea pigs, dogs and marmosets, by injection of infected material or of pure cultures of the organism is described, but we are not yet told how the pure cultures were obtained. There follow numerous other papers in which the whole subject becomes more and more confused. In Paper VI ("Cultivation, morphology, virulence and biological properties of Leptospira icteroides." *Ibid.*, *30*: 13) isolation of the organism by special culture methods from three of eleven yellow fever patients is reported and its properties are described. As he found that it passed through Berkefeld filters, Noguchi raised the question of its having a granular phase under certain conditions. Examination of fresh blood from yellow fever patients by the dark-field microscope made in more than 27 cases revealed the presence of L. icteroides in three ("VII. Demonstration of Leptospira icteroides in the blood, tissues, and urine of yellow fever patients and of animals experimentally infected with the organism.." *Ibid.*, p. 87). Finally in another paper ("The mosquitoes in relation to yellow fever." *Ibid.*, p. 401) Noguchi concludes "The foregoing experiments show that symptoms and lesions closely resembling those of yellow fever in man may be induced in guinea pigs by the bite of female Stegomyias that have previously sucked the blood of a yellow fever patient. . . ." Within the next few years more papers by Noguchi appeared and others also claimed to confirm his work until Beeuwkes, Stokes, and Bauer and their associates working on yellow fever in West Africa completely failed to isolate Leptospira from their cases or to reproduce the disease in small animals (Ref. 15). There is an excellent critical review of Noguchi's work in Klotz' De Lamar Lecture (Klotz, Oscar: *Yellow Fever in West Africa*. Baltimore, The Williams and Wilkins Co., 1928).

15. Stokes, Adrian, Bauer, J. H., and Hudson, N. Paul: "The transmission of yellow fever to Macacus rhesus." Preliminary note." *J. A. M. A., 90*: 253, 1928.

The authors working under the West African Yellow Fever Commission of the Rockefeller Foundation opened the modern era of yellow fever research by showing beyond question that the disease could be regularly transmitted to M. rhesus monkeys both by injections of blood from patients and by bite of infected mosquitoes. The disease could also be passed from monkey to monkey both by blood and by mosquito bite. Mosquitoes were infective 16 days after feeding on an infected animal and remained so until death. Stokes unhappily acquired yellow fever and died before this paper was published.

There shortly appeared by the same authors a full report of their work ("Experimental transmission of yellow fever to laboratory animals." *Am. J. Trop. Med., 8*: 103, 1928). They showed in addition that serum passed through Berkefeld and Seitz filters was infective although a filtrate of infected mosquitoes macerated in salt solution failed to produce disease. The specific pathological findings in infected monkeys are reported in detail. They also showed that convalescent serum in doses of 0.1 cc. protected monkeys against infection while 2 cc. of normal human serum fails to protect. Numerous other phases of the subject are discussed.

The circumstances of this epoch-making work are graphically told by Wilbur A. Sawyer ("Recent progress in yellow fever research." *Medicine, 10*: 509, 1931).

Stokes, Bauer, and Hudson's work was soon confirmed by C. Mathis, A. W. Sellards, and J. Laigret ("Sensibilité du Macacus rhesus au virus de la fièvre jaune." *Compt. rend. Acad. d. Sc., 186*: 604, 1928) who isolated in monkeys at Dakar the famous "French" strain which was much used in later experimental work.

16. Bauer, Johannes H.: "The transmission of yellow fever by mosquitos other than Aedes aegypti." *Am. J. Trop. Med., 8*: 261, 1928.

It had heretofore been thought that Aedes aegypti was the only mosquito which could transmit yellow fever. Bauer found in West Africa that "A. luteocephalus and A. africoannulatus transmitted this disease in all respects in the same manner as A. aegypti," thus opening the subject of multiple vectors. A full discussion of the complicated subject of arthropod vectors of yellow fever is given in Strode (Ref. 29, p. 233 et seq.).

17. Sawyer, W. A., Kitchen, S. F., Frobisher, Martin, Jr., and Lloyd, Wray: "The relationship of yellow fever of the Western Hemisphere to that of Africa and to leptospiral jaundice." *J. Exper. Med., 51*: 493, 1930.

There had been great uncertainty whether "yellow fever" in various localities was the same disease. In South America for example Noguchi had isolated a leptospira (Ref. 14) which was not obtainable in West Africa by Stokes and his associates. Sawyer and his associates collected strains of virus from all over the world and by cross protection tests showed that "The yellow fever now in South America, the present yellow fever of Africa and the historic yellow fever of Panama and other American countries are the same disease." They also showed that there was no cross immunity between yellow fever and leptospiral jaundice.

18. Cowdry, E. V., and Kitchen, S. F.: "Intranuclear inclusions in yellow fever." *Am. J. Hyg., 11*: 227, 1930.

The discovery of intranuclear inclusion bodies of a sort common in diseases due to filtrable viruses was of the greatest importance in the etiologic study of yellow fever. Such bodies were early described by C. M. Torres ("Inclusions nucléaires acidophiles (dégénérescence oxychromatique) dans le foie de Macacus rhesus inoclué avec le virus Brésilien de fièvre jaune." *Compt. rend. Soc. de biol., 99*: 1344, 1928) and by Cowdry and Kitchen ("Intranuclear inclusions in yellow fever." *Science, 69*: 252, 1929) but the present impressive study is definitive. The inclusions are described in great detail and are illustrated by beautiful plates. They were found in liver cells of both infected monkeys and in human material and are regarded as specific. No signs of any such bodies were found in infected mosquitoes. "In conclusion . . . the nuclear response in yellow fever is of the same general type . . . which occurs in many other virus diseases, especially chickenpox, herpes, virus III disease and submaxillary disease."

19. Bauer, Johannes H., and Mahaffy, Alexander F.: "Studies on the filtrability of yellow fever virus." *Am. J. Hyg., 12*: 175, 1930.

Although Stokes, Bauer, and Hudson (Ref. 15) had reported preliminary experiments on filtrability of yellow fever virus, their work was incomplete, and Bauer and Mahaffy studied the subject systematically. They found the virus readily passed through filters without marked diminution in potency; serum was infective at time in amounts as small as

0.0000001 cc.; lack of infectivity of a filtrate of mosquitoes ground in salt solution was due to toxicity of the diluent for the virus; no evidence was found to indicate that virus in blood differs from that in mosquitoes.

20. Theiler, Max: "Studies on the action of yellow fever virus in mice." *Ann. Trop. Med., 24*: 249, 1930.

Although transmission of yellow fever to monkeys was a discovery of the greatest importance, monkeys turned out to be difficult to work with and too expensive for extensive use. A smaller animal was necessary. Theiler therefore made a great contribution when he found that yellow fever virus could be established in white mice. It occurred to Theiler to try the intrathecal route; following injection the animals became sick and at autopsy sections of brain invariably showed an encephalitis and specific nuclear changes. Theiler was able to pass the disease indefinitely through mice in this fashion. Infective mouse brain stored at —8° C. retained its virulence for at least 160 days. He was not able to successfully protect mice by injection of an immune serum. The mouse virus produced yellow fever in monkeys. Many other details are discussed.

21. Berry, G. P., and Kitchen, S. F.: "Yellow fever accidentally contracted in the laboratory. A study of seven cases." *Am. J. Trop. Med., 11*: 365, 1931.

In the course of their meticulous studies on "laboratory" yellow fever, Berry and Kitchin, after reviewing the previous literature, report careful counts of the leukocytes. They noted a progressive fall in number from onset to 5th or 6th day. The counts fell as low as 2625, the polymorphonuclear neutrophiles dropping to levels of as low as 500 to 1000. From the sixth to eighth days the total leukocyte count increases, soon reaching normal levels.

22. Sawyer, W. A., and Lloyd, Wray: "The use of mice in tests of immunity against yellow fever." *J. Exper. Med., 54*: 533, 1931.

The need of a test for immunity to yellow fever had become obvious. Following Theiler, Sawyer and Lloyd developed a workable and reliable protection test in mice. The unknown serum to be tested for its protective power is mixed with yellow fever virus, fixed for mice and injected intraperitoneally into mice which have been prepared by an intracerebral injection of starch solution. If the serum is protective the mice remain well; if not the virus localizes in the brain at the site of the starch injection. This

test has turned out to be of great value both in research and in epidemiological studies.

23. Sawyer, W. A., Kitchen, S. F., and Lloyd, Wray: "Vaccination against yellow fever with immune serum and virus fixed for mice." *J. Exper. Med.*, *55*: 945, 1932.

"The method here presented for vaccination against yellow fever was devised primarily to interrupt the long series of accidental infections of persons making laboratory investigations." The writers first review previous unsatisfactory attempts at vaccination and then describe their own method. "After preliminary experiments on monkeys, 15 persons were actively immunized by a single injection of a dried mixture of living yellow fever virus, fixed for mice, and human immune serum, with separate injections of enough additional serum to make up the amount required for protection" . . . "immunity rose in a few weeks to a height comparable to that reached after an attack of yellow fever and remained there through an observation period of 2 months."

24. Lloyd, Wray, Theiler, Max, and Ricci, N. J.: "Modification of the virulence of yellow fever virus by cultivation in tissues *in vitro*." *Tr. Roy. Soc. Trop. Med. and Hyg.*, *29*: 481, 1936.

Although E. Haagen and M. Theiler ("Untersuchungen über das Verhalten des Gelbfiebervirus in der Gewebecultur." *Zentralbl. f. Bakt.*, 1. Abt. Orig., *125*: 145, 1952) had successfully grown a neurotropic strain of yellow fever virus in a medium of minced chicken embryo tissue and serum-Tyrode solution, no one had succeeded in growing an unmodified pantropic strain. This the authors succeeded in doing during a period of 21 months, and through more than 150 subcultures. The complicated technique is described in detail as well as various antigenic characteristics of the virus and the immunological reactions. A little later Hugh H. Smith and Max Theiler ("The adaptation of unmodified strains of yellow fever virus to cultivation *in vitro*." *J. Exper. Med.*, *65*: 801, 1937) succeeded in cultivating yellow fever virus in a medium containing mouse embryo brain tissue.

25. Elmendorf, John E. Jr., and Smith, Hugh H.: "Multiplication of yellow fever virus in the developing chick embryo." *Proc. Soc. Exper. Biol. and Med.*, *36*: 171, 1937.

The writers were first to successfully cultivate yellow fever virus in the developing chick embryo. John P. Fox and Hugo W. Laennert, Jr.

(" The cultivation of yellow fever virus. II. Observations on the infections of developing chick embryos." *Amer. J. Hyg., 46*: 21, 1947) elaborated these observations. With reference to human vaccination they showed that prolonged passage of the 17 D strain did not apparently alter the virus and did not increase neurotropic virulence for monkeys or man.

26. Whitman, Loring: " The multiplication of the virus of yellow fever in Aëdes aegypti." *J. Exper. Med., 66*: 133, 1937.

After reviewing some older and inconclusive work, the authors showed that " following an incubation period, titers of virus can be recovered from mosquitoes which are significantly higher than at any preceding time." They conclude that, " Aëdes aegypti have been shown to be capable of multiplying the Asibi strain of yellow fever virus in their bodies. Following the ingestion of infected blood, the content of virus falls for several days, reaching a minimum during the first week. It then increases rapidly until quantities of virus greater than those previously encountered can be demonstrated." At about the same time L. Whitman and P. C. A. Antunes (" Studies on Aëdes aegypti infected in the larval stage with the virus of yellow fever." *Proc. Soc. Exper. Biol. and Med., 87*: 664, 1938) showed that " Aëdes aegypti can be infected in the larval stage with the virus of yellow fever, providing they are exposed to large quantities of virus. The resultant adults, both male and female are infected."

27. Theiler, Max and Smith, Hugh H.: " The use of yellow fever virus modified by in vitro cultivation for human immunization." *J. Exper. Med., 65*: 787, 1937.

Using a strain of yellow fever virus modified by prolonged cultivation, the authors vaccinated human beings without the simultaneous injection of immune serum. Eight normal persons were used with minimal reactions, and their sera two to four weeks after inoculation showed the presence of yellow fever antibodies. H. H. Smith, H. A. Penna, and A. Paoliella (" Yellow fever vaccination with cultured virus (17 D) without immune serum." *Am. J. Trop. Med., 18*: 437, 1938) grew the virus in chick embryos and prepared a vaccine which they used on a large scale with good results. G. M. Findlay and F. O. MacCallum (" Note on acute hepatitis and yellow fever immunization." *Tr. Roy. Soc. Trop. Med. and Hyg., 31*: 297, 1937) were the first to report a considerable number of instances of acute hepatitis following yellow fever vaccine; they came to no definite conclusions as to cause. With the occurrence of many cases of

acute hepatitis following mass vaccination in the United States Army, W. A. Sawyer, K. E. Meyer, M. D. Eaton, J. H. Bauer, Persis Putnam, and F. F. Schwentker ("Jaundice in army personnel in the western region of the United States and its relation to vaccination against yellow fever." *Am. J. Hyg.*, *39*: 337, 1944; *40*: 35, 1945) took up the subject in earnest and issued a huge report of their elaborate studies, "The most plausible hypothesis as to the source and nature of the icterogenic agent was that it had been introduced into the vaccine in human blood serum secured from supposedly normal donors and used in manufacture of the vaccine, that the agent was most probably an unknown filtrable virus capable of causing disease in man. . . ."

The whole story of induced immunity to yellow fever is told in detail in Strode (Ref. 29, p. 203 et seq.).

28. Pickels, Edward G., and Bauer, Johannes H.: "Ultrafiltration studies of yellow fever virus." *J. Exper. Med.*, *71*: 703, 1940.

The writer already knew the approximate size of the virus particles from the observations from previous ultrafiltration studies (Bauer, J. H. and Hughes, T. P.: "Ultrafiltration studies with yellow fever virus." *Am. J. Hyg.*, *21*: 101, 1935). In the present study they observed, in the ultracentrifuge by optical methods, the behavior of yellow fever virus particles directly in the unaltered serum from infected monkeys, and were able to estimate the size of the particles to be between 12 and 19 mμ. For a detailed discussion of the physical and chemical properties of the virus see Strode (Ref. 29, p. 46 et seq.).

29. Strode, George K., editor: *Yellow Fever*, 1951, New York, McGraw-Hill Book Company.

An impressive compilation dealing with all phases of yellow fever with comprehensive bibliography. An invaluable reference book.

ON HYSTERICAL AND HYPOCHONDRIACAL AFFLICTIONS *

ILZA VEITH

Among the most challenging aspects of the history of medicine is the examination of ideas that left no mark on contemporary developments and failed to constitute a basis for medical progress subsequent to their inception. That this should extend to a physician of the stature of Thomas Sydenham is astonishing, for his writings, comprising but one medium-sized volume, have always been easily accessible and his fame has been such that even his contemporaries knew him as "the English Hippocrates." While his writings on gout, chorea, and epidemic diseases have been read and quoted by generations of physicians, Sydenham's account of hysteria remained almost entirely unnoticed.[1] Even two centuries later when the definition of hysteria brought additional fame to Charcot, Sydenham's anticipation of many of Charcot's discoveries received hardly any attention.[2] Still today, little can be found in the writings by historians of psychiatry of Sydenham's contribution to their field, although his biographer Payne had reported on it in 1900.[3] In view of the present interest in matters relating to psychiatry it seems desirable to point renewed attention to that aspect of Sydenham's works, particularly since he had elaborated on it upon the request of his colleagues.

* Read at the twenty-eighth annual meeting of the American Association of the History of Medicine, Detroit, Michigan, May 12, 1955.

[1] It is interesting that Richard Mead (1673-1754) although a professed admirer of Sydenham and his work did not refer to the latter's account and interpretation of hysteria. While quoting widely from Sydenham's writings on other matters, Mead's sections on the hypochondriacal and hysterical diseases are entirely traditional in concept. This is all the more surprising, since Mead was familiar with Piso's (see footnote 10) therapeutic precepts on hysteria, and thus, in all probability, also with Piso's etiological considerations. See the posthumous collection of *The Medical Works of Richard Mead, M. D.*, London, 1762, pp. 180-82, 196, 555-57, and 565.

[2] It appears that Sydenham's recognition for his insight into hysteria was limited to two French authors around the turn of the century. G. Abricossoff in her *L'hystérie aux XVII^e et XVIII^e siècles (Étude historique et bibliographique)*, Paris, 1897, pp. 40-49, introduces Sydenham's thoughts on the subject, largely in the form of quotations. Henri Cesbron in *Histoire critique de l'hystérie*, Paris, 1909, pp. 81-86, deals similarly with Sydenham's writings on hysteria, without, however, giving much stress to one of Sydenham's major points, i. e., the recognition of hysteria in men. For the latter reference I am indebted to Dr. Owsei Temkin.

[3] Joseph Frank Payne: *Thomas Sydenham*, London, 1900, pp. 142-146. See also H. Haeser: *Lehrbuch der Geschichte der Medizin und der epidemischen Krankheiten*, Jena, 1853, Vol. 1, pp. 624-625.

In the last quarter of the 17th century there was a rumor among some English physicians that Dr. Thomas Sydenham had developed a new insight into the definition and treatment of hysteria. He had not published his findings and his colleagues became anxious to receive this information while Sydenham's frail health still permitted him to put his thoughts on paper. Finally, on November 17th, 1681, Dr. William Cole (1635-1716) of Worcester, himself a medical author of considerable repute, addressed a letter to the great physician urging him to communicate his observations "concerning the so-called hysterical diseases," which had long exercised the wits of physicians and eluded the recognized methods of treatment.

Sydenham's reply of January 20th, 1681-2, shows that he was much pleased with the deferential inquiries of his noted colleague and quite ready to reveal his thoughts on this disease which were indeed novel to the medical profession of his day. Early in his *Epistolary Dissertation*, which served as an answer to the " most illustrious and learned " Dr. Cole, Sydenham stated that he had been led to the treatment of hysteria because he had observed that next to fever it was the commonest of diseases.[4] This statement concerning the widespread incidence of hysteria must have astounded the 17th century physicians who had not learned to recognize it except in its most blatant stages. More astonishing even must have been his later elaborations where he points out that not only women but men, too, can be counted among its victims. I am quoting these sentences verbatim, for no paraphrase could do justice to their clarity of definition and their sagacity of observation:

> Of all chronic diseases hysteria—unless I err—is the commonest; since just as fevers—taken with their accompaniments—equal two thirds of the number of all chronic diseases taken together, so do hysterical complaints (or complaints so called) make one half of the remaining third. As to females, if we except those who lead a hard and hardy life, there is rarely one who is wholly free from them—and females, be it remembered, form one half of the adults of the world. Then, again, such male subjects as lead a sedentary or studious life, and grow pale over their books and papers, are similarly afflicted; since, however, much, antiquity may have laid the blame of hysteria upon the uterus, hypochondriasis (which we impute to some obstruction of the spleen or viscera) is as like it, as one egg is to another. True, indeed, it is that women are more subject than males. This, however, is not on account of the uterus, but for reasons which will be seen in the sequel.[5]

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[4] *The Works of Thomas Sydenham, M. D.*, translated from the Latin Edition of Dr. Greenhill with a Life of the Author, by R. G. Latham, M. D. London, 1848, vol. II, p. 54.

[5] *Ibid.*, p. 85. All of Sydenham's subsequent quotations are taken from the " Epistolary Dissertation " *loc. cit.*

This initial statement is followed by numerous examples of the various physical disturbances that are produced by hysterical disposition in adult males, females, and even in children. Among these, Sydenham lists recurring violent headaches, occasionally followed by vomiting, convulsive coughing, and spasms of the colon. In pointing to pains that resemble those of nephritis, Sydenham cautions that here differential diagnosis is difficult: "It may be hysteria or it may be a calculus; and unless there have been some antecedent mental emotion . . . the former may be mistaken for the latter." This uncertainty he found even more pronounced in those cases where pain in the bladder and retention of urine occurred. In connection with "hysteria on the stomach" Sydenham observed continuous vomiting and diarrhea.

From the hysterical afflictions of the internal organs he proceeds to describe those of the external parts, "the muscular flesh of the jaws, shoulders, hands, legs and ankles, sometimes causing pain and sometimes swelling." Even some forms of toothache are not exempted from his list of hysterical complaints, and "of all pains . . . the most certain [to be of hysterical nature] is the pain in the back." Diuresis he considers an almost unfailing symptom of hysteria in females and of hypochondriasis in males.

Of equal perspicacity is Sydenham's description of the mental sickness that concurs with the physical disturbances. He speaks of the incurable despair of the patients, their belief that they must suffer all the evil that befalls mankind, their presentiment of further unhappiness. He describes their propensity to anger, jealousy, and suspicion and points to their very occasional intervals of joy, hope, and cheerfulness. Their daytime moods are mirrored by their dreams, which are haunted by sad forebodings. "All is caprice," he says, "they love without measure those whom they will soon hate without reason."

With deep compassion for these patients, Sydenham compares their physical and mental sufferings to life in a purgatory wherein they expiate and pay the penalty of crimes committed in a previous state. Yet he emphasizes that these men and women are not insane, and he even goes so far as to say that "those who thus suffer are persons of prudent judgment, persons who in their profundity of meditations and the wisdom of their speech far surpass those whose minds have never been exerted by such stimuli."

It is obvious that some of the mental symptoms described above are a composite of the modern conception of both hysteria and depression. But in view of the dearth of information on mental disease in the 17th century,

Sydenham's failure to distinguish hysterical manifestations from the depressions he observed in some of his hypochondriacal patients is hardly surprising. His eagerness to prove the identity of the two diseases and his inexperience in making differential diagnoses may also account for his somewhat extravagant statement that, next to fever, hysteria was the most frequent of diseases.

Sydenham's explanation for the cause of hysterical manifestations and their diversity is extremely interesting. He believed that an imbalance of the "animal spirits" [6] upset the equilibrium of the mind-body relationship and caused disturbances in that part of the body that happened to be weakest at the moment.[7] The framework of the mind he described as far more delicate than that of the body "a structure consisting in the harmony of eminently excellent and almost divine faculties; so, whenever the constitution of the same shall, by any means, have become interrupted and broken down, the ruin will be great . . ." Being entirely certain of the correctness of his etiological reasoning, Sydenham states emphatically:

> It is clear then, to me, that it is not any corruption of either the semen or the menstrual blood . . . to which this disease is to be referred. It is rather the faulty disposition of the animal spirits. There is no malignant halitus to the parts affected, no perverse deprivation of the juices, no congestion of acrid humours. There is the cause I have assigned, and no other.[8]

The extent of Sydenham's perceptiveness in regard to hysteria, or rather his dual entity, hysteria-hypochondriasis, can be appreciated only if we compare his views with those of his predecessors and contemporaries. As he himself points out, antiquity had laid the blame of hysteria on the uterus. And for two subsequent millenia the term hysteria with its implicit uterine involvement was not re-examined, but accepted to mean physical disease, caused by the displacement of a freely migrating uterus. Even Soranus who concerned himself extensively with questions of gyne-

[6] It is extremely difficult to find a satisfactory explanation of the meaning of the term "animal spirits" (*spiritus animales*). Although the term originated in antiquity, its meaning shifted constantly, and Sydenham himself seems to have been somewhat uncertain about the nature of these spirits to whom he attributed such far-reaching influence. He conceived of them as dominating both mind and body, for "it is from the irregularity of the spirits that the inconsistency both of mind and body . . . so prevalent with both the hysterical and hypochondriacal take birth. The strength and constancy of the mind . . . depends most especially upon the . . . spirits which are at the top of the scale of matter, and on the very verge of the immaterial entity."

[7] Centuries later this was described by Freud as "das organische Entgegenkommen in der Neurose."

[8] *The Works of Thomas Sydenham*, p. 95.

cology and obstetrics and whose insight into psychiatric disorders by far surpassed that of his contemporaries did not have a moment's hesitation in accepting the traditional theories of hysteria. While he admitted that catalepsy, apoplexy, epilepsy, and even the affliction with intestinal worms often resembled "hysterical suffocation," he pointed out that the latter "always involved an ascent of the uterus . . . which appears to bulge out above the navel or abdomen . . . and usually occurs in adult life after an affection of the uterus."[9] Although both Soranus and his translator Caelius Aurelianus had some knowledge of and much interest in the female anatomy, they were blinded by tradition to the discovery of the stationary quality of the uterus.

The middle ages contributed little to the study of the anatomy of the uterus and even less to a dispassionate study of hysteria. However, the continued belief in the physical origin of hysteria may have saved the lives of a number of women with this affliction, for thus they were spared the fate of exorcism and torture, often meted out to those considered obsessed.

Sydenham was by far the most explicit but not the first to express the belief that hysteria could extend to both sexes. Among those who preceded him in this insight was Carolus Piso who in 1618 ventured the thought that hysteria originated in the brain and was therefore not restricted to women.[10] But the medical profession, bound by the literal meaning of the term hysteria, paid little attention to Piso's statement.[11] Indeed, more than two centuries later, and after Charcot had demonstrated hysteria in males at the Salpêtrière, some physicians insisted that since *hystera* was the Greek term for uterus, it was impossible for men to be hysterics. This criticism of his literal-minded colleagues Sydenham escaped by describing the masculine version of hysteria as hypochondriasis.

Sydenham's choice of hypochondriasis as a corresponding illness to hysteria is very deliberate, for hypochondria was a familiar concept in the late 17th century. Following Smollius' publication in 1610,[12] this

[9] *Caelius Aurelianus*: *On Acute Diseases and On Chronic Diseases*, edited and translated by I. E. Drabkin. Chicago, 1950, pp. 173, 175, 333, 485, 887.

[10] *Selectiorum observationum et consiliorum*. See also: A. Levy: *La vie et l'oeuvre médicale de Charles le Pois* (Carolus Piso). Nancy, 1914.

[11] Sydenham does not mention Piso's writings. Since he appears careful in citing the sources of his information, it can be assumed that Sydenham was not familiar with Piso's statement on hysteria.

[12] G. Smollius: *Trias maritima, proponens per introductionem trium aegrotantium, sororum morbosarum, domesticarum, hypochondriacae spleneticae; hypochondriacae meseraicae; hypochondriacae phantasticae; ortum et interitum.*

disease had become the subject of many treatises, in which the hypochondrium was accorded a similar rôle to that of the uterus in hysteria.[13] Just as " uterine suffocation " was held to give rise to many physical and mental disorders in women, so *suffocatio hypochondriaca* was believed to cause numerous derangements of the abdominal viscera and, specifically, of the spleen.[14] The last-named involvement was considered to be the cause of melancholy which frequently concurred with the physical complaints.[15]

Although hypochondriasis was not explicitly restricted to the male sex, it was generally diagnosed in men, whereas similar complaints in women were classified as hysteria. Sydenham's description of hypochondriacal melancholia (as quoted above among the mental symptoms concurring with both diseases) is strongly remindful of the modern definition of depression. In Sydenham's day the word hypochondriasis did not yet contain the connotation of a morbid preoccupation with physical health. It appears that this was added by Jean Pierre Falret,[16] who was the first to describe false beliefs as to an impaired state of health as one of the characteristics of hypochondria.

It may be assumed that Sydenham was anatomically and physiologically more enlightened than most of his contemporaries, and that his extensive readings and his own experiences had made him aware of the fact that the theory of the migrating uterus was without anatomical foundation. His writings, however, make it evident that he did not reason on the basis of his anatomical knowledge, but that he drew his conclusions from the similarity of the behavior pattern he had observed in his male and female patients.

It is a matter of conjecture whether an argument based on anatomical facts might have made Sydenham's views on hysteria and hypochondriasis more acceptable to the physicians of his day. As it was, his reasoning seems to have reached far beyond his tradition-bound colleagues. Thus, his contributions towards the understanding of the disease were not taken up

[13] C. Van Diik: *De suffocatione hypochondriaca.* Lugd. Bat. 1665. R. Westhoff: *De affectu hypochondriae.* Argentorati, 1668. H. G. Herfelt; *De affectione hypochondriaca,* Duisburgi ad Rhenum, 1678, etc.

[14] N. Highmore: *Excercitationes duae quarum prior de passione hysterica; altera de affectione hypochondriaca.* Oxon, 1660. T. Willis: *Affectionum quae dicuntur hystericae et hypochondriacae, pathologia spasmodica vindicata. . . .* Lugd. Bat. 1671, etc.

[15] The 11th edition of the *Encyclopaedia Britannica* still lists " the spleen " as synonymous to hypochondriasis.

[16] *De l'hypochondrie et du suicide.* Paris, 1822.

by his contemporaries, nor does he appear to have influenced later workers in the field.[17]

Actually, Sydenham himself was by no means free from the fetters of tradition, as is evidenced by his therapeutic measures which seem oddly out of step with his brilliant observations on the origin and the nature of hysterical afflictions. After having eulogized the intricate workings of the mind whose imbalance caused physical deterioration as well as mental suffering, it is startling to find Sydenham's treatment of hysteria and hypochondriasis to be directed entirely to the physical symptoms. He bled and purged his patients and then followed this up with medicines that "fortified the blood." These medicines he compounded from a selection of a vast variety of vegetable substances, for, as evidenced by opium and quinine, he felt that nature supplied specifics for all diseases. Occasionally, he added iron filings to these vegetable compounds.

To be sure, Sydenham's therapeutic measures were based on careful reasoning. It was his belief that hysteria and hypochondriasis, though by no means lodged in the humors, begat an accumulation of putrid humors which impaired the organs designed to purify the blood and thus caused the physical afflictions of hysteria and hypochondriasis. Bleeding and purging were thus intended to relieve the body of impure blood before it could do further harm, and the fortifying remedies were to bring about a complete reversal of the pathological process that had caused the disease.

While it cannot be assumed that true hysteria was cured by the therapy described above, it appears likely that Sydenham's intensive concern for his patients and their physical and mental sufferings had the ameliorating effect which he ascribed to his tangible ministrations. Moreover, while his remedies *per se* lacked novelty, his attitude in dispensing them differed greatly from traditional practices.

Sydenham was totally devoid of rigidity and adjusted his treatment to the special requirements of each case. Not only did he recommend great caution in regard to bleeding and purging, he even refrained completely from their use in cases of physical debilitation. With the same caution he administered his blood-building remedies. He was aware of individual reactions to different medicines and cognizant of the fact that some hysterics had so strong an idiosyncrasy as to feel an absolute repugnance

[17] One notable exception is Cotton Mather whose *Angel of Bethesda* contains quotations and paraphrases of Sydenham's views on hysteria. *Cotton Mather: First Significant Figure in American Medicine* by Otho T. Beall, Jr. and Richard H. Shryock. Baltimore, 1954. P. 146.

against his "hysterical pills." In such cases he omitted medication altogether, relying entirely on "the prince and pattern of physicians . . . Time."

At first glance Sydenham's treatment of hysteria may seem anticlimactic after his description of its nature; but on closer study we see that only deep insight into the psycho-physical processes of this disease could have led him to say with Hippocrates: "With Nature against us, all is in vain."

ON THE INTERRELATIONSHIP OF THE HISTORY AND THE PHILOSOPHY OF MEDICINE *

OWSEI TEMKIN

Anybody wishing to speak about the philosophy of medicine will, at the outset, encounter two difficulties: the vagueness of the term and the prejudice against the subject itself. To some degree, the two difficulties are related to each other. With the spread of positivism in the nineteenth century, philosophy became synonymous with speculations that lead nowhere. Today, too, this attitude is often met with among scientists, physicians, and historians, let alone so-called practical men. We may admit that the prejudice does not altogether lack a foundation. Undisciplined philosophical thinking can camouflage more easily than can undisciplined reasoning in the sciences or history where results carry their own evidence.

The careless manner in which the word " philosophy " is bandied about is an additional sign of the slight importance attached to it. Not infrequently the same person who disparages philosophy will also use the word where no philosophy is really involved. Thus we hear of a philosophy of this or that where no more is implied than a theory, opinion, or belief. To some extent this carelessness is in line with the general disregard for the meaning and weight of words. We read of the symptomatology of a case or of its methodology where the author has merely recorded the symptoms of a case or indicated the method he used. And indeed, if it is possible to speak of pathology in the stomach, then there is nothing remarkable in speaking of a philosophy of appendectomy.

Yet it cannot be doubted that all through its history medicine has received philosophical attention in a more than casual manner. Some of the great classics of medicine embody a true philosophy: several of the treatises that go by the name of Hippocrates, some chapters in Sydenham, Claude Bernard's *Introduction to Experimental Medicine*. We feel that, in a manner not always easily defined, these books deal with the fundamentals of medicine and are of a philosophical nature. Curiously enough, their influence has been greater than that of many writings which carry the name of philosophy of medicine or otherwise make their philosophical aim explicit. Thus it would seem that philosophy of medicine can be

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seen in a twofold way: as a broad line of thought that goes beyond scientific, clinical, or technical details, without claiming a philosophical title; or as the consciously philosophical treatment of medicine. Let us now discuss the interrelationship between the history of medicine and this general philosophical approach first, and turn to philosophy in the stricter sense afterwards.

If we look for a combination of unprofessional philosophy with medical history, we may easily think of William Osler. He referred to himself as "a plain man whose life has never been worried by any philosophy higher than that of the shepherd in *As You Like It*." [1] There is no doubt that Osler was well acquainted with the tenets of quite a number of philosophers, even if their thoughts never bothered him. His imperviousness to full dress philosophy was countered by his belief that "every man has a philosophy of life in thought, in word, or in deed, worked out in himself unconsciously." [2] Osler himself not only possessed such a philosophy, but I venture to think that the force with which he expressed his "way of life," largely accounts for his influence in the Anglo-Saxon world. He told others what they should think and feel, and the history of medicine was a powerful medium for his teaching. Osler received inspiration from old books and thus he told the budding physicians to read the journals and the old books and to leave the new books to the old men.[3] When looking at his *Evolution of Modern Medicine* or his other historical writings, my admiration for his wide culture is mingled with the feeling that the past largely reflects his own individuality, that he does not expect us to learn from historical investigation in the same sense in which he would expect a physician to learn from scientific research. "It is one of the values of lectures on the history of medicine," he writes, "to keep alive the good influences of great men even after their positive teaching is antiquated. Let no man be so foolish as to think that he has exhausted any subject for his generation." [4] Having heard these words, who will dare to admit to such foolishness, even if Osler had not bolstered his warning with examples of the influence of Virchow, Lister, and Ehrlich?

[1] William Osler, *A Way of Life*, reprinted Baltimore, Remington, 1923, p. 3.

[2] *Ibid.*, p. 1. On Osler's relation to philosophy, especially to William James and Pragmatism, cf. Ludwig Edelstein, "William Osler's Philosophy," *Bull. Hist. Med.* 1946, *20*: 270-293.

[3] William Osler, "Internal Medicine as a Vocation," *Aequanimitas; with Other Addresses to Medical Students, Nurses and Practitioners of Medicine*, Philadelphia, P. Blakiston, 1904, p. 149 f.

[4] William Osler, *The Evolution of Modern Medicine*, New Haven, Yale University Press, 1921, p. 219.

Osler's " way of life " included the history of medicine. And since Osler viewed himself as a humanist, his understanding of medical history was humanistic too. The humanist sees history as made by men, and Osler certainly saw medical history as made by men, above all by the great physicians. But his biographical approach was not without a theoretical foundation. He defined history as " the biography of the mind of man " and thought that " its educational value is in direct proportion to the completeness of our study of the individuals through whom this mind has been manifested." [5] This note, even though Osler may not sound it often, should not be entirely missed. It does not make Osler a Hegelian (even though he mentions Hegel in this connection), but it shows that the history of medicine to him had a substratum underlying its biographical diversities.

History taken as the revelation of the human mind can also be approached in a different way. It should be possible to write a history of medicine that traces the development of medical thought with relatively little concern for bibliographical details or priorities of inventions and discoveries, but with emphasis " on the connection between general civilization and medicine and on the development of medical thinking." [6] These words constitute the program with which Max Neuburger prefaced his *History of Medicine*. At first sight, Osler's *Evolution of Modern Medicine* and Neuburger's *History of Medicine* seem as different as the two authors themselves. Neuburger was not a man of ringing imperatives; he was a quiet scholar who tested the old medical systems for their philosophical content. Moreover, his aim to follow the connection between general civilization and medicine continued a tradition of medical historiography that went back to the days of Daniel LeClerc. Correlating the development of medicine with philosophical systems would have been nothing new. But Neuburger's effort went in a different direction. He tried to find the common philosophical denominator for the medicine of a period and other manifestations of its cultural life. As an example we may quote his work on the healing power of nature [7] in which he showed how belief or disbelief in this power was connected with philosophical doctrines on the one hand and principles of medical practice on the other. It is not merely the wish to honor the memory of a great medical historian

[5] *Ibid.*

[6] Max Neuburger, *Geschichte der Medicin*, 2 vols., Stuttgart, Ferdinand Enke, 1906-1910; vol. 1, p. v.

[7] Max Neuburger, *Die Lehre von der Heilkraft der Natur im Wandel der Zeiten*, Stuttgart, Ferdinand Enke, 1926.

who died just two months ago that makes me pay tribute to Max Neuburger in this discussion of the interrelationship of the history and philosophy of medicine.

Whoever pursues the history of medicine in a philosophical vein will tend towards Osler or Neuburger. The direction will depend on whether he considers history as a medium of expressing his philosophical beliefs or as the body of material incorporating the philosophical beliefs of past generations. The two attitudes are not exclusive; they may both be present, though to different degrees.

So far we have spoken of looking at medicine and its history in a philosophical vein. Prudence would advise us to stop here. But then we would not do justice to our subject which includes the philosophy of medicine, *sensu strictiori*. What does this mean? In 1947, Professor Szumowski read a paper before the International Congress of the History of Science with the title "The Philosophy of Medicine, Its History, Essence, Designation, and Definition." [8] This paper contains an interesting account of the history of the philosophy of medicine and is a valuable guide to the publications in the field. This is Professor Szumowski's definition:

> The philosophy of medicine is a science which considers medicine as a whole. It studies its position in humanity, in society, in the state and in the medical schools. It embraces at a glance the whole of the history of medicine. It reveals the more general problems of the philosophy of biology. It analyzes the methodological form of medical thought, mentioning and explaining the errors in logic which are committed in medicine. It borrows from psychology and from psychics the knowledge and the ideas which are of importance in the totality of medicine. It touches upon medical "praxeology," discusses the chief values in medicine, formulates ethical principles and medical deontology. And, finally, it discusses medical aesthetics.[9]

Though this seems to me an overly generous definition, I shall not try to improve on it. Instead, I suggest that, in analogy to general philosophy, the philosophy of medicine should present us with a medical logic, medical ethics, and medical metaphysics. This will lead us on to somewhat firmer ground, at least as far as logic and ethics are concerned. Medical logic is a subdivision of general logic; it is a chapter in methodology, dealing with the concepts that constitute medicine, their meaning as well as correct use. Medical ethics is a part of general ethics. Just as the latter

[8] W. Szumowski, "La philosophie de la médecine, son histoire, son essence, sa dénomination et sa définition," *Arch. Internat. d'Hist. des Sci.* 1949, *2*: 1097-1139.

[9] *Op. cit.*, p. 1138.

teaches what is good and bad, medical ethics, presumably, should teach what is good and bad in medical situations.

We have facilitated our task by omitting, for the time being, medical metaphysics. Even so, the definitions we gave are merely formalistic. In essence they say that philosophy of medicine teaches the principles of right thinking and acting in medicine. What relationship does this have to the history of medicine which deals with what has been done and thought in medicine? In answer we may quote a passage from Thomas Aquinas: "The purpose of philosophy is not to know what men have thought, but what is the truth of things." [10] If we apply this general statement to our particular problem, it means that philosophy and history of medicine pursue different aims and that their connection cannot be an intrinsic one. As a matter of convenience, the putting together of medical history, philosophy, sociology, and literature may be justified from the administrative point of view of a medical school. Moreover, they often need one another's help, as we hope to show later in the case of history and philosophy. But before establishing good neighborly relations, we must bring the differences into focus, which we may do by reference to medical logic on which we possess several books, Blane's *Elements of Medical Logick* which first appeared in 1819, and Oesterlen's [11] and Bieganski's [12] works of the same title. None of these is a historical book nor does history play a pre-eminent part in the first two named, except by way of illustration. It was Blane's intention "to expound the physiology, pathology, and therapeutics of the medical mind," because he deemed it necessary that physicians "discipline their minds, by a knowledge of the laws of evidence, and the rules of investigation." [13] Oesterlen, who came some thirty

[10] *De Caelo et Mundo* (Bk. 1, lectio 22): "Studium philosophiae non est ad hoc quod sciatur quid homines senserint, sed qualiter se habeat veritas rerum."—Quoted from *Aristotles' De anima, in the Version of William of Moerbeke and the Commentary of St. Thomas Aquinas*, translated by K. Foster and S. Humphries with an introduction by Ivo Thomas, London, Routledge and Kegan Paul, 1951, p. 20.—As any textbook of historical methodology will show, the relationship of history and philosophy in general has long been an object of discussion. However, the problematic nature of a philosophy of medicine and the specialized task of medical history have made it appear to me more profitable to refrain from simply placing the present discussion within the general framework.

[11] F. Oesterlen, *Medical Logic*, translated and edited by G. Whitley, London, Sydenham Society, 1855.

[12] W. Bieganski, *Medizinische Logik*, autorisierte Übersetzung nach der zweiten Original-Auflage von Dr. A. Fabian. Würzburg, Curt Kabitzsch, 1909. The book originally appeared in Polish.

[13] Gilbert Blane, *Elements of Medical Logick*, second edition, London, Thomas and George Underwood, 1821, pp. 15 and 14.

years later, stood on the shoulders of John Stuart Mill's *Logic*, and Bieganski, the German translation of whose book appeared in 1909, stood under the influence of experimental medicine. But in spite of the diversity due to the different situations under which the books were written, the underlying aim was the same: to establish correct thinking in medicine. The past yields examples of fallacies and illustrates the ways of progress. But even in citing examples from past experience, we must have a standard of fallacy and progress. The same is true of medical ethics. At first, this may sound surprising. Is it not true that medical ethics of our day largely rests on the Oath of Hippocrates, on Percival's code, and other historical documents? Should we then not benefit by their study? We certainly shall benefit in as far as our understanding of present-day medical ethics is concerned. One may even go further and claim that it is impossible to reach any understanding without a study of the historical development. Yet, understanding of the way things have come about is not the same as judging them correct and right. Historical understanding is one thing, philosophical judgment another.

The point has been made that the difference in character between history and philosophy has been confused by medical historians.[14] Allegedly, history and philosophy are sciences in their own right.[15] I believe that this implies an oversimplification of the issue, since on the authority of outstanding philosophers we must at least admit that philosophy may not be a science at all. It is for this reason that the two are separate and yet interrelated. Philosophy is not identical with any science and yet related to all of them. Where, then, does the relationship to the history of medicine lie?

Let us suppose that we wish to define the notion of health. How shall we go about it? To find a definition of some validity we first look up the word "health" in a dictionary and find: "soundness of body and figuratively of mind," etc. Obviously we must now look up the meaning of "sound," and we read: "free from disease or defects or blemishes or corruption or heresy." In other words, health is freedom from disease. And what is disease? "A serious derangement of health." We should not blame the dictionary for such tautology since dictionaries are meant to give nominal definitions only. Besides, the dictionary introduced a somewhat puzzling distinction which we should not overlook. It referred

[14] Cf. A. W. Kneucker, *Richtlinien einer Philosophie der Medizin*, Wien, Maudrich, 1949, pp. iii, 17.

[15] Kneucker, *op. cit.*, pp. iv, 18 and *passim* speaks of "exakte wissenschaftliche Philosophie" and the transfer of its laws to medicine.

to health as "soundness of body" and "*figuratively* of mind." Does this mean that a body only can be healthy or diseased whereas, strictly speaking, these terms do not apply to the mind? Surely we not only speak of mental disease, we even insist that a mentally diseased person should be regarded on the same plane as, say, other sick human beings. There is then a discrepancy between the explanation of the dictionary and our own current view. Who is right? The decision will rest on what is meant by body and mind. If we conceive of the mind as a spiritual entity, entirely independent of a physical body, we may indeed wonder how it could become diseased. There existed a school of psychiatry that doubted the possibility of mental disease and therefore considered psychiatric disorders as fundamentally due to somatic disturbances. The opposing school explained mental diseases in moral or religious terms because the mind lives in the sphere of good and evil, of saintliness and sin.[16] It is true that Juvenal once said: "mens sana in corpore sano esto"—but did he think of the mind in the same purely spiritual way as did Descartes, who accentuated the difference more than anybody else had done before?

Thus, instead of resolving the argument, we have introduced historical considerations. What did these historical data contribute toward a possible clarification of the issue? They put it on a much broader basis. We found that the dictionary, echoing older views, was at variance with our idea of health which we took for granted. Now we no longer take it for granted; we have become aware of a philosophical problem. Without this awareness, our starting point would have been one-sided. I do not say that the historical way is the only one by which to reach this awareness. But it is, perhaps, the shortest. It is in this sense that we can quote Goethe's famous dictum, "dass die Geschichte der Wissenschaft die Wissenschaft selbst sei." To this he added: "One cannot clearly recognize one's own possessions until one knows how to recognize what others possessed before." [17]

[16] On the difference between the "somatologists" and "psychologists" cf. S. Cornfeld in Neuburger-Pagel's *Handbuch der Geschichte der Medizin*, vol. 3, Jena, Gustav Fischer, 1905, pp. 675-681, and Gregory Zilboorg, *A History of Medical Psychology*, New York, W. W. Norton, 1941, pp. 434 ff. and 464 ff.

[17] Zur Farbenlehre. Didaktischer Theil, Vorwort (*Goethes Werke*, vol. 33, Stuttgart, Cotta, 1881, p. 7). One might also cite here Fuller's dictum, "That history maketh a young man to be old without either wrinkles or grey hairs; privileging him with the experience of age without either the infirmities or inconveniences thereof," which Osler quoted in his preface to Max Neuburger, *History of Medicine*, translated by Ernest Playfair, vol. 1, London, Oxford University Press, 1910, p. vii. (Dr. W. W. Francis kindly informs me that Dr. Osler was quoting from the preface of Thos. Fuller's *History of the Holy War*, 1639.)

There is yet another point we have to consider. Our students are taught medicine at our schools. What do we mean thereby? We mean that they are taught a certain number of facts, theories, methods, and skills which are considered valid and useful for preventing and treating disease. It is silently assumed that we impart the accumulated knowledge of the ages, that is, what has proved endurable and sound in the work of former generations. I sometimes wonder at the assurance with which this assumption is made. On whom do those who are not interested in historical work rely when they take it for granted that everything worth while preserving has entered into modern texts? Do they rely on medical historians? Flattering as this would be, I can hardly believe that such great trust is placed in us. Do they rely on the working of progress, taking it for granted that in the process of time mistakes were constantly eliminated while truth was built on truth? I cannot quite share such confidence. The medical work of Cotton Mather that has recently been brought into relief by Beall and Shryock shows how generations may condemn what will later appear a step in the right direction. Nevertheless, it would be foolish to exaggerate the addition to practical knowledge that might accrue from historical studies. But I do deny that our students are taught medicine in the fullness of the word. Medicine deals with man, and the picture of man they receive is a partial one.

The above consideration makes us aware of a whole field of medical philosophy that we have not mentioned yet, namely medical anthropology. By this is not meant medicine of the so-called primitive tribes, but the way in which a physician looks at man.[18] Now, the medical anthropology of Hippocrates, Paracelsus, and William Osler, to take three representative figures, was not the same. The Paracelsian picture of man as a microcosm to be judged by cosmic analogies was not shared by the Greek physician or the doctor of the late nineteenth century. Nor did either of them have the firmly rooted conviction that the physician was merely God's instrument. Though for Hippocrates too nature was divine, she was not the all-powerful creator of Heaven and Earth. And the leading physicians of the time of Osler were too much imbued with the idea of man-made progress to share the religious humility of Paracelsus and Ambroise Paré.

Such fundamental differences are not subject to the filtering process of scientific progress, since they are not products of accumulating knowl-

[18] See e. g. Luis S. Granjel, "Introducción a la Antropología Médica," *Imprensa Médica*, Lisboa, 1953 (reprint).

edge and refinement of methods. They belong in the same category as artistic styles and great philosophical thoughts. They are not just moments in an historical development, but remain as possibilities of experiencing and comprehending the world.

It may be objected that such a comprehensive view of medicine, based on the memory of whatever medical ideas existed in the world, is irrelevant. Even a philosophy of medicine should be a guide for us, not an abstract survey of logical or ethical possibilities. To give an example: any definition of medicine that starts out with " medicine is a science . . ." is incomplete, for there were and are nations that based their practice of medicine on traditional procedures. It might even be wrong to say that medicine is the relationship between a physician and his patient, because such a relationship is not necessarily present where a medicine man performs his rites over a sick person in the presence of the tribe. Nevertheless, we do not intend to go back to the days of unscientific medicine and we do not wish to give up the patient-physician relationship. Why, then, should we not start from where we stand? I do not presume to say what should or should not be taught in our schools. In my own limited experience the attempt at gaining an insight into the medical past, just where it appears strange, has helped me most toward understanding our present position. I view the current trend to concentrate on what is close to us with some alarm. " English *Humores* are not Hungarian, nor the Neapolitan Prussian, wherefore must thou go where they are; and the more thou seekest them out and the more thou learnest of them, the greater is thine understanding in thy native land." [19] What Paracelsus here said of the study of disease in foreign lands holds equally well of the medical study of man at different times.

Altogether we must not forget that philosophy of medicine, just as history of medicine, is not the exclusive property of the medical profession. We can therefore not decree where philosophical or historical inquiries should begin or end. And here we come to the final question regarding the interconnection of the two. The physician, general historian, or sociologist who writes medical history will have a point of view based upon the present status, aspirations, and functions of medicine. How, then, will a philosopher write medical history? The nature of his vocation makes it impossible for him to start out with a definite picture of what medicine is, since

[19] " Seven Defensiones," translated by C. Lilian Temkin in *Four Treatises of Theophrastus von Hohenheim, Called Paracelsus*, edited by Henry E. Sigerist, Baltimore, Johns Hopkins Press, 1941, p. 27.

he wants to find out what it ought to be. As it so happens, the oldest history of medicine was written by Menon upon the request of his teacher, Aristotle. As far as its fragmentary remains allow us to judge, it constituted an outline of the various medical systems known to Menon. Such a collection probably was to serve Aristotle's usual procedure when philosophizing on a subject. He reviewed what others had thought not for historical purposes, but to utilize it for his own doctrine, and thus arrive at the truth. A modern philosopher would not be able to approach his task in the simple manner of Menon. In order to know what others had thought he would have to turn historian in the technical sense of the word. To quote Goethe again:

> One will only really understand how a person thinks about a definite point when one knows the general constitution of his mind. This is true if we are desirous of apprehending exactly the opinions concerning scientific matters, whether of simple individuals or of whole schools or centuries. For this reason the history of the sciences is intimately bound up with the history of philosophy, but equally so with the history of the lives and characters of individuals and of peoples.[20]

Old books yield their meaning only if read in the context of their time. And the context of the time would primarily mean the picture of man, health, and disease which that time possessed.

But there would be more to it. Our philosopher's task would be more difficult than that of the ordinary historian. Professionally doubtful of all ready-made explanations, he would have to be wary of the many conceptual helps which we constantly use such as "influence," "advance," "social, economic, and political forces," "climate of opinion." Decidedly, we need not envy our philosopher-historian. Not only would he have to watch his step where most of us tread lightly, he would also have to penetrate into metaphysical depths which most of us avoid.

There comes to my mind a passage which I read in a very thorough work on medical ethics written half a century ago: ". . . We shall look upon death as the gravest result of disease or of a drug. It is true, death is not always considered the worst evil. But even though we would otherwise agree with the poet that 'life is not the greatest of all good things,' for the medical profession there can be only one rule, that life is the supreme good, death the worst evil." [21]

We shall easily agree with our colleague who wrote this passage. But

[20] Materialien zur Geschichte der Farbenlehre. Erste Abtheilung, Betrachtungen über Farbenlehre und Farbenbehandlung der Alten (*op. cit.* vol. 35, p. 10).

[21] Albert Moll, *Ärztliche Ethik. Die Pflichten des Arztes in allen Beziehungen seiner Thätigkeit*, Stuttgart, Ferdinard Enke, 1902, p. 235.

our philosopher will have to ponder about the remarkable fact that men have set up in their midst a profession charged with an idea of supreme good and evil that is admittedly not the common idea. How is this possible? Where the foundation of medical ethics, just as the foundation of medical logic merges into the metaphysical question of the whence and why of medicine, I must confess my uncertainty whether we are still in the realm of history. Perhaps it is better to leave this question to the philosopher who, today more than ever, is eager to explain our existence, and to await his answer.

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MEDICO-HISTORICAL NEWS AND ACTIVITIES

CORRESPONDENCE AND REPORTS

DR. REUBEN FRIEDMAN

1892–1956

Dr. Reuben Friedman was born in Philadelphia on December 14th, 1892, and died in the Temple University Hospital on February 14th, 1956. He graduated from Central High School in 1912 and from the Temple University School of Medicine in 1916. In 1922 and 1923 he studied at The University of Vienna, receiving a certificate for his studies in General Medicine. In 1930 and 1931 he attended the Graduate School of Medicine of the University of Pennsylvania, specializing in Dermatology, and in August, 1931, joined the staff of the Department of Dermatology at The Temple University School of Medicine. He started as Instructor in Dermatology and at the time of his death was Professor of Clinical Dermatology in this department.

While Dr. Friedman published many articles dealing with various dermatological subjects, his main interest in writing was on the subject of medical history. Four of his books were devoted to scabies. These were *The Emperor's Itch. The Legend Concerning Napoleon's Affliction With Scabies* 1940; *Scabies—Civil and Military. Its Prevalence, Prevention and Treatment* 1941; *Biology of Acarus Scabeii* 1942, and *The Story of Scabies* 1947. He also wrote and published several monographs on scabies, two of which, dealing with Giovan Cosimo Bonomo, the discoverer of the parasitic nature of scabies, came to the attention of the Italian Society of Dermatology and Syphilology with the result that he was made a corresponding member of that Society and was honored in Livorno, Italy, on May 14th, 1950, when a re-dedication of the Memorial to Bonomo took place. Exhibits on scabies by Dr. Friedman at the American Academy of Dermatology and Syphilology meeting in Chicago in 1940 and at the annual meeting of the American Medical Association in Cleveland in 1941, were noteworthy. At the Chicago meeting he also demonstrated photographically the "Scrape and Smear Method" for the microscopical diagnoses of typical and atypical scabies which he had previously devised.

In 1955 Dr. Friedman published his final book, *A History of Dermatology in Philadelphia*, which won wide acclaim. Through the generosity of a pharmaceutical company, a copy of this book was sent to every medical library in the United States.

The Temple University Medical Alumni Association recognized the excellence of Dr. Friedman's work in the field of dermatology, and in 1949 he received the Distinguished Alumnus Award.

In addition to holding the title of Professor of Clinical Dermatology in the Temple University School of Medicine, Dr. Friedman was Professor of Derma-

tology at the Temple University School of Chiropody, visiting lecturer in Dermatology at the Graduate School of Medicine of The University of Pennsylvania, attending dermatologist to the Temple University Hospital and Vice-Chairman of the Medical Board and consulting dermatologist to the Skin and Cancer Hospital. He was a Fellow of the College of Physicians of Philadelphia, member of the American Association of the History of Medicine, the Academy of Dermatology and Syphilology, the Società Italiana di Dermatologia e Sifilografia, Sociedad Cubana de Dermatologia y Sifilografía, and the Sociedad Cubana de Cancerologia. He was also a member and past president of the Philadelphia Dermatological Society and was president of the Temple University Medical Alumni Association for the years 1946-1948. At the Temple University School of Chiropody, Dr. Friedman was sponsor of the Pi Alpha Pi Chiropody Fraternity. He was a member of Congregation Adath Jeshurun, Philadelphia.

In addition to his wife, Henrietta, Dr. Friedman is survived by a daughter, Mrs. Harold G. Ross of Takoma Park, Maryland; a son, PFC Ralph B. Friedman, Fort Huachuca, Arizona, and a sister, Mrs. Dora Gassel, Philadelphia.

CARROLL S. WRIGHT

ANNOUNCEMENTS

NATIONAL NEWS

Duke University

A dedication ceremony of the Trent Room in the Duke Medical Library and of the Josiah C. Trent Collection of books and manuscripts in the history of medicine took place on April 20, 1956. Named after the late Dr. Josiah C. Trent, surgeon and well-known medical historian and collector, the collection was the gift of Mrs. James Hustead Semans. The ceremony was incorporated into the program of the American Association of the History of Medicine which was holding its annual meeting at Durham, N. C., April 19-21, 1956. Dr. John F. Fulton was the principal speaker at the ceremony.

Johns Hopkins University

The Johns Hopkins Medical History Club held a meeting on May 14, 1956, at which Dr. Percival Bailey of the University of Illinois presented his impressions of Harvey Cushing in a paper entitled " Pepper Pot."

NEWS FROM ABROAD

International Congress of the History of Medicine

According to a communication from Dr. Palafox, the Fifteenth International Congress of the History of Medicine will take place in Madrid and Salamanca, September 21-29, 1956. The brochure issued by the Congress gives the composition of the Executive Committee as follows: P. Lain Entralgo (President), F. Oliver and L. S. Granjel (Vice-Presidents), S. Palafox (Secretary), A. Albarracín (Treasurer), R. Roldán, G. Folch, Lasso de la Vega, A. Castillo, J. A. Paniague, J. J. Girona. The program foresees the following topics: (1) the Iberian Peninsula and Arabic Medicine, (2) Medical Relations between the Iberian World and the Other Countries of Europe, (3) Medical Iconography in the Sixteenth Century, (4) Other Subjects. Papers can be submitted until June 30 to the General Secretariat, Duque de Medinaceli 4, Madrid. Maximum speaking time is ten minutes.

Tentative plans for the Congress include an opening session in Madrid, September 22, and scientific sessions, September 24-27, excursion to Toledo on September 23, visit to the Escorial and arrival in Salamanca on September 29 where the concluding session will be held. Registration fees for participants in the Congress who are not members of the International Society of the History of Medicine are 500 pesetas, for members, 300 pesetas, and for accompanying persons, 250 pesetas. The brochure contains other information of interest to those planning to attend the Congress.

BOOK REVIEWS

ERWIN H. ACKERKNECHT. *A Short History of Medicine.* New York: The Ronald Press Company, 1955. xviii + 259 pp., Ill. $4.50.

A short history of medicine was badly needed. We have a profusion of long ones but, outside of Charles Singer's *Short History* of 1928, we have no presentation that covers the field in such brevity. Hence, this *History* fills a real gap and I can well imagine that it is going to be a most popular student's textbook.

Of course, when one is astrained to such brevity one cannot tell the whole story and has to skim the surface as of necessity. But the author has succeeded in presenting the history of medicine in a well-balanced way that makes fascinating reading. The first chapters competently cover the fields of paleopathology, paleomedicine and primitive medicine, to which Ackerknecht has contributed numerous papers, which we hope to find some day assembled in a volume. Next follows a chapter on medicine in the ancient civilization of Egypt and Mesopotamia and the world historical outlook is represented by a chapter of ancient India and China. Three chapters are devoted to Greek medicine, one each to medieval medicine, Renaissance medicine, medicine in the 17th, and medicine in the 18th century.

The 19th century has been split up into a number of attractive chapters such as cover the basic sciences, clinical medicine, the rise of bacteriology, of surgery and of the various specialties. A particularly satisfactory chapter is devoted to public health and professional developments in the 19th century and an equally satisfactory one to medicine in the United States prior to 1900. And the book ends up with a brilliant analysis of the main trends in the first half of the 20th century.

More progress has been achieved in the 20th century than in the whole course of history but Ackerknecht justly points out that " in spite of the unprecedented achievements of this century, some of the most prominent and ' modern ' traits of present-day medicine actually were already well in evidence in the nineteenth century." And I might add they were well in evidence in the 18th century, which was in my opinion a very great century, with the discovery of electricity and the invention of the steam engine that initiated the Industrial Revolution, and with the philosophy of Enlightenment that had far-reaching results in the field of public health and hygiene. The 20th century did merely reap the harvest sown by the preceeding centuries, and this is why the history of medicine makes such fascinating reading and should be taught in every medical school.

Ackerknecht gives as part of an Appendix a list of the Nobel Laureates in Medicine and Physiology to illustrate some of the 20th century trends. What strikes one most in this list is a marked shift toward the Anglo-Saxon countries. We also become aware of the fact that the time of the " great doctors," such as were Paracelsus, Boerhaave, Osler, Clifford Allbutt or Sahli, is irrevocably over. Perhaps the last great doctors were Friedrich von Müller and the surgeon

Sauerbruch or in France Chauffard, Widal or the surgeon Leriche. Progress in medicine is achieved nowadays in the laboratories by specialists who are medical scientists.

The second part of the Appendix, one which will be greatly welcomed by students—so we hope—contains suggestions for further reading. A judicious selection is made of books recommended that are easily available in America.

All in all this is a well-rounded, well-balanced book which we wish to see in every medical student's hands.

HENRY E. SIGERIST

ROBERT MONTRAVILLE GREEN. *Asclepiades: His Life and Writings.* A Translation of Cocchi's *Life of Asclepiades* and Gumpert's *Fragments of Asclepiades.* New Haven, Conn. Elizabeth Licht, Publisher, 1955. ix + 167 pp. $6.00.

The jacket and title page of this work display "the marble monument whereby Asclepiades is represented." It is tempting to compare this with the portrait contained in the figurated woodcut border with which Hans Weiditz adorned Brunfels' "Catalogue of Illustrious Doctors" of 1530. The Weiditz portrait—the face of a hardened sceptic with a sardonic, almost cynical smile—seems ever so much more appropriate than the expressionless head from the Capitoline Museum in Rome. This bears the inscription of a name which may have belonged to anybody, including a number of physicians called Asclepiades.

The great Bithynian reformer who called Hippocratic medicine a "meditation of death" fittingly had his vogue at the time of the French Revolution, although the *Life of Asclepiades* by Antonio Cocchi (1695-1758)[1]—here reprinted—appeared as early as 1762 (in an English translation) and was followed by another Italian treatise (Biacchini, *La medicina d'Asclepiade per ben curare le malattie acute*) in 1769. The fragments, however, were not collected until Christian Gottlieb Gumpert published in 1794 the essay which forms the main part of the present book.

Inevitably Asclepiades' revolutionizing doctrines attracted the followers of John Brown—such as the physiologist and "Naturphilosoph" K. F. Burdach who at the age of 24 devoted his first book to a parallel between Asclepiades and John Brown (Leipzig 1800). It was preceded by his learned thesis: "Dissertatio sistens scriptorum de Asclepiade recensionem." Burdach's book affords most interesting sidelights and would have provided an excellent companion to the treatises of Cocchi and Gumpert here published. We cannot agree with the statement of the editor that "not since 1841, when Ritter von Welz published at Würtzburg his 'Des Asclepiades von Bithynien Gesundheitsvorschriften,' has a book on this important physician been printed" (p. viii). We only refer to H. von Vilas,

[1] It may be noticed in passing that his "discourse delivered at Florence" (August 1743) on the *Pythagorean Diet of Vegetables only, Conducive to the Preservation of Health and the Cure of Diseases* contained a reference to scurvy being due to the absence of fresh vegetables in the diet. The book was translated and appeared in London in 1745 (French translation, The Hague, 1762).

Der Arzt und Philosoph Asclepiades von Bithynien (1903) and to Wellmann's fundamental paper (*N. Jahrb. d. klass. Altert.* 1910, XXI, 684-703). However, Gumpert's collection and most learned essay remain of great value; they have become rare and their reprinting in English must be greatly welcomed—particularly when presented in a volume as readable and elegant as the book under notice.

WALTER PAGEL

A. C. CROMBIE. *Robert Grosseteste and the Origins of Experimental Science, 1100-1700.* Oxford: Clarendon Press, 1953. xii + 369 pp. Ill.

This book maintains an interesting thesis with skill, learning and a richness of documentation which in itself is of great value to the reader. Plan and arrangement of the book can best be given in the author's own words (p. v). He "tried to show first [i. e. ch. 1 and 2] how the particular intellectual and practical interests of Western thinkers, especially from the twelfth century, led them to ask particular kinds of questions about the natural world, in consequence of which they came to create the modern experimental method. In the thirteenth century the Oxford school, with Robert Grosseteste as its founder, assumes a paramount importance: the work of this school in fact marks the beginning of the modern tradition of experimental science. The first half of the book [i. e. chs. 3-7] is devoted to Grosseteste and the Oxford school; the second [chs. 8-11] deals with their influence on the spread of the experimental method in western Christendom and with its history down to Newton." Since "a special place in the interests of Oxford was taken by the study of optics and of the rainbow," the author "used the history of these subjects as practical examples of the experimental method in operation."

Mr. Crombie does not fail to draw attention to Greek philosophers, and scientists from Aristotle on who made the "first steps" toward a theory of induction and experiment (p. 8). In particular, he points out the role played by ancient medicine in this connection. Thus the fact that Grosseteste in a decisive characterization of the experimental procedure (pp. 72-74) refers to scammony suggests to Mr. Crombie that Grosseteste had in mind a section in Avicenna's *Canon* which gives the rules "that must be observed in the recognition of the virtues of medicaments by way of experiment" (ed. Venice, 1608, I, 246; Crombie p. 80). Crombie lists these rules (p. 80 f.) and says that "they were taken partly from Galen" (p. 79 where in note 7 Galenic references are given). Moreover, Crombie knows that "the original medical source of the method of resolution and composition [elaborated by Grosseteste] was Galen himself" (p. 77), viz. in the beginning of the *Ars medica*, a treatise upon which Alī ibn Ridwān, in the eleventh century, wrote a commentary popular among Western physicians.

As to the role of scammony, we may add that the belief in its cholagogic power was already expressed in the Hippocratic treatise "On Internal Diseases" 51 (ed. Littré, vol. 7, p. 296). But the question as to how the drug could exert such a specific action remained a lively one into the Renaissance. Even Peregrinus

in his *De magnate* vented it (Crombie, p. 208, note 1). An answer was given by the (pseudo?) Galenic *De teriaca ad Pisonem liber*, ch. 3 (ed. Kühn, vol. 14, p. 223) which speaks of "simple drugs of which some act by their whole substance and show a single and simple power like scammony which seems to draw the yellow bile." Scammony did thus enter into the class of drugs whose faculties, according to Galen, cannot be brought within a methodical, rational system but "can be known through experience only" (ed. Kühn, vol. 12, p. 192).

In thus reemphasizing and elaborating the relationship between Grosseteste's logical formulations and Greek medicine, one may question the author's statement that for the Greeks "geometry was the model not only for their astronomy, optics, and mechanics, but also for their biology and medicine" (p. 4). In their pharmacological theory, as we have seen, they admitted the necessity of a different approach. And it seems to me that the same is true of their descriptive anatomy. Mr. Crombie's impression of the significance of Grosseteste's logical formulations is largely derived from his analysis of works on optics and magnetism. But if approached from the aspect of the medical sciences, the same ideas appear largely as logical elaborations of procedures developed in antiquity. No doubt the contributions of medieval medicine have been, and still are, underestimated. Yet it would not be easy to point to any methodological advances over the ancients. If Crombie cites the Salernitan *Anatomia porci* as exhibiting the interest of medieval scholars "in practical scientific problems" (p. 18), then, by comparison, the *Administrationes anatomicae* make Galen a giant "in the practical arts" (*ibid.*).

Not so very long ago science in the middle ages was represented as smothered under a mass of theological and metaphysical speculations by men more intent upon the salvation of their souls than on the exploration of nature. Now there is an increasing tendency to see in medical philosophy and science a practical orientation in contrast to the contemplative attitude of the ancients. This trend seems to me as one-sided an approach as its former opposite. Unless I am mistaken, it is largely due to our own increasing bias in favor of the technical side of science. On the one hand, this has led to an awareness of medieval technology. But on the other, it has tempted many scholars to view the medieval relationship between technology and science somewhat in the manner of the twentieth century.

It is only fair to add that Mr. Crombie's point of view does not intrude itself in a manner that would detract from the value of the book for those who do not share his general assumptions about ancient and medieval science. He has not only utilized a large amount of source material, but has also interpreted it thoroughly and presented it lucidly. Moreover, his bibliography will be used with benefit by anybody interested in medieval science. Altogether, this is one of the most important publications in the field that have appeared in recent years.

OWSEI TEMKIN

ELMER BELT. *Leonardo the Anatomist.* Logan Clendening Lectures on the History and Philosophy of Medicine. Fourth Series. Lawrence, Kansas: University of Kansas Press, 1955. 76 pp. $2.00.

In the opening quotation to *Leonardo the Anatomist* the author has, I believe, purposely selected the well chosen words of da Vinci himself to inform the reader that the pages to follow will depict realistically and artistically the anatomy of the human body in terms of structure and function.

Dr. Belt has made an organized and critical analysis of the anatomical studies of Leonardo. In regard to pathological anatomy the blood vessels of the old and the young are compared in the illustration on page 28. Leonardo noted in his dissection of the vessels of the aged that they were thickened and irregular in contour (arteriosclerosis), while those of youth were not thickened or twisted.

From his studies of mechanistic anatomy he realized that muscles were balanced, and used rope strands to indicate the function of these structures. Almost everywhere functional anatomy is dominantly present, and is most strikingly shown in the illustration on page 23 in which he demonstrates how the coronary arteries receive their blood supply from the aortic valves during diastole.

That the sum of the parts go to make the whole is well recognized by da Vinci for much of his anatomical work deals with parts of anatomy. The ventricles of the brain of the ox, for example, were injected with a solidifying medium and hence their extent and shape were investigated.

Da Vinci did not overlook regional anatomy which is well demonstrated in his studies of the genito-urinary system. About forty per cent of the book is devoted to this group of organs. But this is quite understandable for the author is not only a distinguished urologist with international recognition, but he is also widely known for his interest and authoritative knowledge of the life and works of Leonardo da Vinci. In this section the author brings out the results of Leonardo's studies of the interrelationship between genito-urinary system structure and function. Sometimes this is erroneously accomplished by Leonardo, but one must emphasize that the knowledge of this system has been generously advanced since the turn of the sixteenth century. For example the author points out (p. 45) that in Leonardo's coition figure one finds two structures taking origin from the inferior portion of the spinal cord and these carry spiritual power to the two channels through the penis which contributes the more viscid portion of the ejaculate. However, most of his anatomical findings and judgments are impressively sound (p. 49); "Here the urethra is shown as a single channel. Into it the ejaculatory ducts from the seminal vesicles empty beyond the sphincter fibers of the bladder neck." As the author stressed, the prostate is not shown in any of Leonardo's drawings, which is strange in view of the fact that he had apparently dissected the genito-urinary tracts of aged men. Perhaps the illustration on page 50 is from a younger male and the prostate was not a prominent structure. The section on the genito-urinary system also contains many informative translations, and these are well selected and suitably placed amongst the subject matter.

Furthermore, one may go to the field of embryology (p. 63) and note that in

Leonardo's judgment the male urethra is the homologue of the vagina, and the seminal vesicles are the homologue of the uterus. And even the domains of genetics and heredity are invaded, for the author recorded the following observations by Leonardo (p. 57); "if black gets black with child in Scythia, the offspring is black; but if a black gets a white woman with child the offspring is grey. And this shows that the seed of the mother has power in the embryo equally with that of the father."

Thus the author has succeeded most effectively, through his forceful style of writing and the use of pertinent data, in placing Leonardo in a position that is surrounded with the immensity of his knowledge of anatomy and allied subjects. In this book containing seventy-six pages, which is in keeping with the occasion for which it was prepared, the subject matter is presented with clarity, factually, and to the point. *Leonardo the Anatomist* deserves to enjoy wide appeal.

JOHN A. BENJAMIN

EMILE LAGRANGE. *Monsieur Roux*. With a Foreword by F. Duran-Reynals. Bruxelles: Ad. Goemaere, 1954. 251 pp. Ill.

More than a biography of Pasteur's collaborator and successor, this book is the sketch of an era that saw the inception and flourishing of bacteriology and its sister sciences. The emphasis is on the participants and on the development of the concepts that emerged from their investigations. Roux's role is projected in sharp focus on the scene.

Born in the small town of Confolens, in Charente, Roux was orphaned at an early age and was brought up in the near poverty of a large family by his eldest sister and her husband. Throughout his life he had to struggle against the restrictions imposed in his early years by his lack of means and in his later years by his poor health. His medical studies were completed at the cost of a committment to serve ten years in the army; this committment he never kept. His studies did, however, serve to bring him into contact with Duclaux and it was probably at this time that his interest in scientific investigations was aroused. Later, in Paris, he had to engage in private practice in addition to other jobs but renewed contact there with Duclaux who was already associated with Pasteur. Roux's first contribution with Pasteur was in the anthrax experiments at Pouilly-Le-Fort; his and Chamberland's work ensured the success of these studies and got Pasteur off the limb. His next work was on cholera in Egypt. Little was accomplished but Koch managed to culture the organism later shown to be the cause of the disease. Roux had a number of other studies under way when Pasteur's interest in rabies was aroused but he did not take an immediate major part in this work. It was, however, his procedure for attenuation that Pasteur adopted, developed, and soon applied to humans. There were some deaths in treated subjects and a public polemic on the efficacy and safety of the procedure arose, similar to one seen some 60 years later about another disease. Roux, who had been opposed to the application to humans, shouldered the further responsibilities for this work when

Pasteur became incapacitated. His relationship with Pasteur and with other colleagues was not an entirely happy one. He treated Pasteur's speculation and conclusions, too often based on inadequate data, with open scorn and disdain; to others, Roux was authoritarian and highly critical to the point of discouraging further work. He dispersed his efforts in too many fields; in Lagrange's opinion he never gave full rein to his talents in spite of his many later contributions.

Though Duclaux became the first director of the newly created Institut Pasteur, it was Roux who, in effect, assumed the responsibility. As a teacher of bacteriological techniques and concepts he was admirable but as an administrator he left a lot to be desired. A misogynist, with few personal needs, with an almost total lack of interest in other than scientific matters, and with a distressing lack of imagination even in scientific investigations, Roux was not able to bolster the Institut financially, in its physical development, or even with new personnel. He had little need for money and did not recognize the needs of others. Lagrange quotes a letter from Roux to the effect that if scientists are to be made to work they must not be paid; the salaries at the Institut were in accord with that philosophy. In contrast, he was fundamentally kind, helped many of his staff financially and did attract the loyalties of most of those who worked with him. His scientific work during these years is too well known to require mention.

Lagrange has written a critical evaluation of Roux's life and points out the contrast of his substantial scientific achievements and of his shortcomings as an administrator. To those who seek a fresh and enlightening account of this era of scientific progress and of the personalities involved, this book should be of great interest.

FRANCIS P. CHINARD

RICHARD H. SHRYOCK. *The Unique Influence of the Johns Hopkins University on American Medicine*. Copenhagen: Ejnar Munksgaard, 1953. 77 pp. Dan. Kr. 17.50.

This slender volume by Dr. Shryock, in surprisingly small compass, gives us in concise and lucid style the essential elements in the great story of the stirring events surrounding the founding and subsequent course of Johns Hopkins University. Like all developments which involve pioneering and revolutionary changes, controversies and conflicts were stirred up which have endured even to the present day.

The author, in literary "capsule" form, makes a strong case for the priority of Johns Hopkins leadership in an amazing number of advances which has brought American medicine to today's high level. Our attention is called to the singularly fortunate timing and combination of circumstances which gave Hopkins its great opportunity. Although the seeds of graduate education had begun to sprout in other American centers, to Johns Hopkins University came the first real opportunity through the gift of a large endowment fund (for that day) and the site on which to create a teaching hospital along completely original lines.

We are reminded of the remarkable wisdom and good fortune of the University's

Board of Trustees in selecting Daniel Coit Gilman as first President, and then, under his guidance, drawing together one of the most remarkable groups of original thinkers and educational leaders the medical world has ever seen. The incomparable John S. Billings not only designed the Hospital, but laid down the fundamental plans to be followed in building a great medical school on a true graduate basis. The "big four," Welch, Osler, Halsted, and Kelly, were selected with unerring judgment to make full use of the virgin opportunity presented for the first time on the American scene.

Up to that time, American medical education had been preoccupied, almost solely, with the *transmission* of learning. Now, the stage was set for a "pilot" university project on a graduate basis, with ample opportunities for full-time professors to engage in research, not only in the medical sciences, but in medical education itself.

The results of the bold venturing at the Hopkins Medical School; requirement of a bachelor's degree for admission, limitation of classes to small size, "the view that students should think for themselves and learn by doing," introduction of the student to the bedside as a responsible member of the "team," straight internships, long-term residencies, and the encouragement of research—provided the pattern on which the famous Flexner Survey was made and the subsequent reforms of medical education throughout the country were made possible. It gave the newly created Council on Medical Education and Hospitals of the American Medical Association the opportunity to take the role of leadership for which it was created. It paved the way for the Association of American Medical Colleges to acquire equal stature.

Other achievements cited in this little book are those of the concept of social service, development of biometry, and the introduction of the pioneering School of Hygiene and Public Health and the Institute of the History of Medicine. The resultant contributions to public health research and training, biostatistics, and the fostering of an interest in the historical and cultural aspects in medicine have been of worldwide value. "Popsy" Welch, in his "four careers," was directly and indirectly the guiding genius who made so much of this unique story come true. One of the unexpected dividends of Mr. Johns Hopkins' investment was the role played by Osler's medical textbook on inspiring Gates to persuade Rockefeller to initiate the series of benefactions by a number of men of wealth in the United States, through which great centers of medical education, research, and community service have grown up in several parts of the country. Thereby, the American ideal of private philanthropy balancing public support, has been widely maintained.

JEAN A. CURRAN

J. H. Van den Berg. *The Phenomenological Approach to Psychiatry.* An Introduction to Recent Phenomenological Psychopathology. Springfield, Ill.: Charles C Thomas, 1955. ix + 105 pp. $3.00.

Van den Berg's "The Phenomenological Approach to Psychiatry" is the second introduction to phenomenological psychology, psychiatry and psychotherapy to appear in this country in little more than a year and, as far as I know, ever. The other was Sonnemann's excellent—though rather difficult—*Existence and Therapy.* Phenomenological psychiatry, according to van den Berg, shares with "phenomenology," its philosophical counterpart, the desire to overcome Cartesian modes of thought. It aims to bring into the open the unrealized philosophical prejudices of the observing psychologist. It returns to direct observation of mental phenomena, as they appear, before the rational reflection of the observer has divided his object into dichotomies, such as mind and body, individual and world, etc. Theoretical constructs, such as "mind," "consciousness," "the unconscious," etc., are avoided, as are causal, finalistic and analogistic modes of thought and mechanical models of mind which treat mental qualities as if they were things. But above all phenomenology is the expression of a new anthropology, a new interpretation of man.

Psychiatric phenomenology is based, in the main, on the work of five men: Jaspers, Husserl, Heidegger, Binswanger and Straus. Jaspers was the first (1913) to develop the phenomenological method as a research tool of psychopathology. To him phenomenology was a descriptive method, meant to give a vivid account of individual mental experiences. Husserl's (1901, 1913, etc.) definition of phenomenology is broader: he calls it "perception of essence." Developing further Brentano's concept of "intentionality" he postulates: (a) the intended object of a perception is the same (psychologically) as the real one; (b) in every observation not only the object of the observation but also its "essence" is observed. E. g.: in listening to someone, we observe not only what he says but also notice the specific character of his voice, etc. Postulate (a) overcomes the idealism-realism problem, postulate (b) the problem of subject-object division, both of them traditional stumbling blocks of the psychologist.

Heidegger (1926), extending Husserl's line of thought, developed concepts which permit us to think of man unseparated from his body, his world, and his fellowmen. For Heidegger, man's basic condition is "being-in-the-world." Being-in-the-world is an "existential," a constitutive ingredient of human nature (others are anguish, understanding, etc.). Being-with, being-in-time, etc., are other basic modes of (human) being.

Binswanger (1923, 1931, etc.) transformed Heidegger's phenomenological method —designed to yield ontological insights—into a method of psychiatric investigation. He looked for "aprioric structures"—analogous to Heidegger's existentials—underlying individual experience and behavior. He found them, above all, in the manifestations of temporalization, i. e., the manifestations of being-in-time. Such manifestations would be, e. g., the slowed-down movements, as well as the experience of "time stands still," in psychotic depression.

Straus (1935), starting from a detailed critique of elemental, behavioristic, and *Gestalt* psychology, developed—independently of Heidegger's ontological investigations—an outline for a new psychology in which man and world are no longer treated as separate entities. He also contributed classical examples of the new approach in his studies on the psychopathological experience of time (1928) and on obsessions (1938, 1948), among others.

A host of Swiss, German, French and Dutch researchers are extending the realm of phenomenological investigation. Their basic assumption is that reality, as the patient sees it, is *the* reality. Broadly speaking, this reality can be investigated in four different ways: in terms of the patient's relations to his world, his body, his fellow-men, and to time. Van den Berg's description of how this is done is adequate but incomplete. He presents the case for phenomenology in terms of a critique of psychoanalysis and he makes some telling points. But he fails to show convincingly why phenomenology would be superior. Psychoanalysis' causal-deterministic, mechanistic interpretation of man is not wrong as such (as van den Berg seems to imply) but inadequate to its object: man. The assumption of psychoanalysis and of all other psychological theories, that man is a thing which can, with appropriate methods, be investigated and completely known like any other thing, is regarded by phenomenological psychiatry as a misinterpretation of man. Van den Berg rightly states that phenomenological psychiatry is a change of attitude, a methodical adjustment, as it were. But he fails to mention the important reasons for a complete anthropological and epistemological reorientation which are contained in the works of Jaspers, Husserl, and Heidegger. He manages, however, to get over the idea that an important trend is developing in psychiatry. Numerous mentions of other authors will help those who are interested to pursue their inquiries at the original sources. A reminder to the editor: terminological oddities, such as "psychical" instead of "mental" should not be permitted to occur with as much frequency as they do in van den Berg's book.

LUDWIG B. LEFEBRE

MEDICAL LIBRARY ASSOCIATION. *Handbook of Medical Library Practice.* With a bibliography of the reference works and histories in medicine and the allied sciences. Second edition. Edited by Janet Doe and Mary Louise Marshall. Chicago: American Library Association, 1956. xv + 601 pp. $10.00.

It is a pleasure to see the second edition of a book which has already become a classic in its field. Although written primarily for the practicing librarian, it has great value for the historian of medicine as well. The editors, who are among the foremost medical librarians in this country, have done an exceptionally fine job, and, as a result, the work has a degree of uniformity and unity unusual in most books of multiple authorship. There are twelve chapters, each obviously written by an expert, covering such factors as personnel, organization, administration, technical services, public services and areas of special interest. The first chapter, "Medical libraries," and the last, "Rare books and history of medicine,"

are recommended especially to medical historians. The bibliography, compiled by Eileen R. Cunningham, Gertrude L. Annan and Mary E. Grinnell, is about four times as large as that in the first edition. Material in appendices in the first edition has either been incorporated into the text or omitted, making this edition more readable than its predecessor. Though the *Handbook* was delayed in publication, very little pertinent material has been omitted.

The chapter on classification, which is the longest in the book, is excellent, as are the ones on reference and cataloging. The latter is heartily recommended to all those who have difficulty finding information in a standard dictionary card catalog. Though a few technical points may be open to question, in general the wealth of detail in all the chapters is presented clearly, with concrete examples and factual data, and will prove of tremendous assistance to those in the medical library field. The chapters on non-book materials and on photoduplication should prove interesting to most readers because they give an indication of the great variety and amount of such material now available in these forms.

The first half of the chapter on rare books and history of medicine (written, of course, by Gertrude L. Annan of the New York Academy of Medicine) is as much a history of medical historiography as it is a guide to the sources. In addition to wise advice on how to build up a good collection of rare and historical books, it contains a section, "Works useful in a historical collection," giving a list of bibliographic tools which should be part of the working knowledge of every medical historian, if only to save his own time and energy. The full bibliographic cataloging described for rare books should prove a Godsend to the librarian who finds himself with a treasure on his hands, particularly since such books are a rarity in all but the largest libraries.

The bibliography consists of a large general section devoted to science, government documents, and medicine, followed by bibliographic material on 105 special subjects, alphabetically arranged. The general section includes history of science, bibliographies for the historian, biographies, and history of medicine, as well as guides to all kinds of other materials which form the primary sources for much of the research in medical history. The subject bibliographies are subdivided by form (bibliographies, indexes, biographies, histories, etc.), which makes them easy to use, and are arranged alphabetically under each form. The whole is adequately indexed. In every part, both general and special, histories of the topic covered are included wherever possible. The bibliography is up-to-date and thorough, and it is annotated throughout, a factor which makes it worth its weight in gold.

No medical library or librarian can afford to be without this book, both for its technical qualities and for its bibliography. For his own sake, no medical historian should deprive himself of the privilege of having such a useful bibliographic tool at hand.

PHYLLIS ALLEN RICHMOND

FRANCIS JOHN HALFORD. *Nine Doctors and God.* Illustrations by Keichi Kimura. Second edition. Honolulu, Hawaii: University of Hawaii Press, 1955. xiv + 306 pp. $4.00. Ill.

Dr. Halford has set down for us all a vivid story about the Hawaiian Islands and the beginning of missionary medicine there. It was back in 1778 that Captain James Cook, the eminent British navigator, sailed into the Waimea River estuary of the Island of Kauai, and startled the natives. Many of them threw themselves prostrate before him, but he scarcely knew that a noted priest had foretold his coming. A year later there was a second expedition, during which a quarrel took place, and Captain Cook lost his life. Fifteen years later only fifteen foreigners had settled in the Islands. Over forty-two years after the discovery of the Islands by Captain Cook there arrived the first group of missionaries sent out from Boston. The earliest of the medical couples, Thomas Holman and Lucia Ruggles, had met and fallen in love in Cooperstown, New York, in September 1819. This is the town remembered as the early home of baseball. Fortunately, this first couple did not endorse the frigid prudishness then so widespread in New England, and everywhere regarded essential to the soul's salvation. Six weeks later they met again in Boston and were married before the deadline set for the sailing on October 15, 1819. Some of this first group had been in attendance at what was commonly called the Mission School in Cornwall, Connecticut. All of them were summoned to the old Park Street Church in Boston to receive orders. They were told what their task was to be. "You are to obtain an adequate knowledge of the languages of the Hawaiian people, to turn them from their barbarous courses and habits, and introduce among them the arts and institutions of civilized life and society." What a commission!

Many friendly Bostonians staged a great farewell reception. One of these had just returned from England and offered to paint the portraits of some of the group. The painter was Samuel F. B. Morse, who invented the telegraph seventeen years later. As they set sail they had to take with them food for twenty-three persons for a six months' journey, together with medicines, farm implements, type and paper for printing, maps of the stars, as well as Bibles and testaments and stationery.

By 9 a. m. on Saturday, October 23, 1819, the first company of missionaries were present on the Long Wharf in Boston. Though blinded by tears, those New England women would not allow their grief to break the shell of their Puritan reserve. Then followed "three bells in the afternoon watch," then the call "all ashore that's going ashore." Later that evening out went the brig from the harbor's mouth at a good eight knots. "Before morning," wrote Lucia Holman, wife of the only doctor abroad, "we were a pitiable company, all down with seasickness."

Dr. Halford takes the story along at a brisk pace, telling of the early rulers that the missionaries found in Hawaii, as well as of the various clashes that occurred among the religious groups. It was a great experience, settling down among the six islands all the way from Hawaii, the major unit, down to Lanai, the smallest, that lay south of Molokai and just west of Maui.

Then followed the later groups, bringing Dr. and Mrs. Blatchely, Dr. and Mrs. Gerit P. Judd, whose descendants are such important citizens today, Dr. and Mrs.

Dwight Baldwin, Dr. and Mrs. Alonzo Chapin, Dr. and Mrs. Thomas Lafon; then Dr. and Mrs. Seth Lathrop Andrews, Dr. and Mrs. James William Smith, and finally, in the ninth company, Dr. and Mrs. Charles H. Wetmore.

It is a great story!

EDWARD H. HUME

RICHARD B. GOLDSCHMIDT. *Portraits from Memory. Recollections of a Zoologist.* Seattle: University of Washington Press, 1956. ix + 181 pp. Ill. $3.50.

As indicated by the title, this book contains the author's reminiscences of some of the great German zoologists whom he knew as teachers or colleagues. Names like Waldeyer, Schaudinn, and Ehrlich are of course familiar to all physicians, and many will also have heard of such men as Bütschli, the brothers Hertwig, Boveri, Kölliker, Haekel, Driesch, Gegenbauer, and others of the great German biologists of the days preceding the first World War. These memories, by one who himself belongs to the masters of his science (he is now Professor Emeritus at the University of California), are not presented in a formal or systematic manner. They preserve the delightfully rambling character of informal seminars in which they originated. This imparts a vividness to the narrative which a systematic presentation might not possess. There emerges a picture of German university life, of its good and bad sides, that is much more instructive than many a writing specifically devoted to that topic.

OWSEI TEMKIN

THE BULLETIN RECENTLY RECEIVED *

Avicenne. *Poème de la Médecine.* Texte Arabe, Traduction Française, Traduction Latine du XIII[e] Siècle, avec Introductions, Notes et Index. Établi et présenté par Henri Jahier et Abdelkader Noureddine. Paris: Société d'Édition "Les Belles Lettres," 1956. xii + 209 pp. Ill.

Esmond R. Long. *A History of the Therapy of Tuberculosis and The Case of Frederic Chopin.* Logan Clendening Lectures on the History and Philosophy of Medicine, Sixth Series. Lawrence: University of Kansas Press, 1956. x + 71 pp. $2.00.

Marvin K. Opler. *Culture, Psychiatry, and Human Values.* The Methods and Values of a Social Psychiatry. With a Foreword by Thomas A. C. Rennie. Springfield, Ill.: Charles C Thomas, 1956. xiii + 242 pp. $6.00.

Charles Singer. *Galen on Anatomical Procedures.* Translation of the Surviving Books with Introduction and Notes. London, New York, Toronto: Oxford University Press, 1956. xxvi + 289 pp. $8.75. (Publications of the Wellcome Historical Medical Museum, N. S., No. 7).

Alfred Vogt. *Theophrastus Paracelsus als Arzt und Philosoph.* Stuttgart: Hippokrates-Verlag, 1956. 212 pp. D.M. 12.50.

Agatha Young. *Scalpel: Men Who Made Surgery.* New York: Random House, 1956. viii + 311 pp. Ill. $5.00.

* The *Bulletin* reserves freedom of decision as to the publications to be listed in this section.

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